MONTHLY NOTICES

OF THE

ROYAL ASTRONOMICAL SOCIETY.

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Rev. T. E. R. PHILLIPS, PRESIDENT, in the Chair.

Lyndon Bolton, M.A., 4 Shakespeare Road, Bedford ;
R. H. Curtiss, Director of the Detroit Observatory, Ann Arbor, Michigan, U.S.A. ;
Wendell Phillips Hoge, Mount Wilson Observatory, Pasadena, Cal., U.S.A. ; and
William Johnson, 4 Lynwood Road, London, S.W. 17,
were balloted for and duly elected Fellows of the Society.

The following candidates were proposed for election as Fellows of the Society, the names of the proposers from personal knowledge being appended :—

Ram Behari, M.A., Lecturer in Mathematics, St. Stephen's College, Delhi, India (proposed by V. M. Gaitonde) ;
Alfred Allinson Bourne, M.A., Prestwich Lodge, Cheltenham, Glos (proposed by W. H. Steavenson) ;
Theodor S. Jacobsen, Assistant in the Lick Observatory, Cal., U.S.A. (proposed by H. H. Turner) ;
Hamilton M. Jeffers, Assistant Astronomer in the Lick Observatory, Cal., U.S.A. (proposed by H. H. Turner) ;
Ernest Alfred Letcher, 815 Higgins Building, Los Angeles, Cal., U.S.A. (proposed by R. Barker) ;
Allen D. Maxwell, James M. Goewey Fellow in the University of California, Berkeley, Cal., U.S.A. (proposed by H. H. Turner) ;
Joseph H. Moore, Astronomer in the Lick Observatory, Cal., U.S.A. (proposed by H. H. Turner) ;

Edward Murray Oakeley, B.A., 157 Banbury Road, Oxford (proposed by H. H. Turner);
Frank Robinson, A.I.N.A., Cunard Steamship Co., Ltd., and 78 Wellbeck Street, Princes Avenue, Hull (proposed by E. Harrison);
Wyman W. Storer, Fellow in the Lick Observatory, Cal., U.S.A. (proposed by H. H. Turner);
Robert J. Trumpler, Associate Astronomer in the Lick Observatory, Cal., U.S.A. (proposed by H. H. Turner);
Cromwell Hall Warren, 23 Florence Park, Redland, Bristol (proposed by L. J. Comrie);
Howard Welch, B.Sc., 35 Century Road, Oldbury, Worcs (proposed by A. J. Potts);
William H. Wright, Astronomer in the Lick Observatory, Cal., U.S.A. (proposed by H. H. Turner); and
Ching Sung Yü, Martin Kellog Fellow in the Lick Observatory, Cal., U.S.A. (proposed by H. H. Turner).

Seventy-one presents were announced as having been received since the last meeting, including, amongst others :—

Astronomy: A revision of Young's Manual of Astronomy, by Russell, Dugan, and Stewart, 2 vols. (presented by H. N. Russell).
A. S. Eddington, Stars and Atoms (presented by the author).
Melbourne Observatory, Astrographic Catalogue, vol. i., zones $-65°$ and $-66°$.

Some Aspects of Astronomical Photography of Precision. (George Darwin Lecture, delivered by Professor Frank Schlesinger on 1927 March 11.)

The purposes for which the George Darwin Lectureship has been established have been described to you. To what has been said I would add a word of personal gratification that my name should be linked in this way, if only for the moment, with that of one of the heroes of science, who was the first to apply a thorough mathematical analysis to the actual problems of cosmogony, a work that will always be regarded as the foundation-stone for an edifice which is itself fundamental in our science. This first George Darwin Lecture might well have been devoted to a review of his work and of its continuation, as carried on by distinguished pupils of his on both sides of the Atlantic. But this is a task that you will hardly expect of me, and I must leave it to some future speaker from this platform, choosing instead a theme upon which I can speak with less presumption.

Many of us in this room can remember the time when photography was struggling for a foothold in astronomy as a method for making accurate measurement. In the early days of the subject and before

proof to the contrary was forthcoming, it was only natural to believe that the direct measurement of objects in the sky, by means of the filar micrometer for example, should give more reliable results than those obtained by so indirect a process as exposing a plate to the sky, immersing it in succession in three kinds of chemical baths, and finally, after it had dried, measuring it in the laboratory. Although more than sixty years have now gone by since photography was first applied to such problems, it is only recently that the photographic plate has come into its own and has achieved general recognition as a precise method. Even to-day the accuracy and the great economy of the results that can be attained in this way are still not as well known as they ought to be. Certainly at the beginning of this century there was still a feeling akin to suspicion regarding the validity of photographic measurement, in spite of the early work of Rutherfurd, and of that brilliant group of men who came together to organise the work of the Astrographic Catalogue. One could give many instances of this feeling at that time, but I shall cite only one. When the Yerkes Observatory began work thirty years ago, Professor Barnard was one of the original members of the staff, and for his principal work with the great refractor, which was at his disposal for two nights in each week, he decided to measure the relative places of a large number of stars in each of four globular clusters, the object being to set down accurate positions from which the relative motions in these objects could be determined by astronomers of the future. By 1900 Ritchey had obtained excellent photographs of these clusters with the same telescope ; but still Barnard, who was as receptive of new ideas as anyone I have known, preferred to continue this work of triangulation by means of the filar micrometer, although he was well aware of the enormous amount of work this decision entailed. In 1903 it was my good fortune to come to the Yerkes Observatory, through the good offices of George Ellery Hale, to try astrometric work by means of photographs secured with the great refractor. I for one had no definite idea what accuracy to expect from such photographs, since up to that time all experience had been confined to plates taken with much smaller instruments, none of these, so far as I am aware, having a focal length as great as one-fourth of that of the Yerkes telescope. To answer this important question as soon as possible, I measured on photographs the same stars in the globular clusters that Barnard was measuring with the filar micrometer on the same telescope. He entered heartily into this experiment and, with his usual kindness, put at my disposal all the data necessary for a comparison. It turned out that the measurement of one image on a plate had about one-third the probable error, and therefore about nine times the weight, of a complete micrometer measurement. I do not know which of us was the more surprised ; perhaps it was I, for Barnard had a very modest notion of all his attainments, and I realized better than he did how high a standard his micrometer work had set. To reach a high plane of accuracy with the micrometer is a difficult matter requiring in any case years of patient practice, and even then is not attained by all. On the other hand, it is a characteristic of the photographic method (I do not know whether to call it an

advantage) that an observer acquires in a few months, or even a few weeks, a standard of accuracy which years of experience do not enable him to surpass ; and further, this standard seems to be about the same for one careful and conscientious observer as for another.

The economy of the photographic method is equally surprising. In the comparison that I have just described, the time spent at the telescope to secure the same result is at least one hundredfold greater with the micrometer, and the measurement on photographs of one hundred stars in each of four globular clusters is a task that can be carried out in a few weeks at most. It is worth mentioning that shortly after we made this experiment Barnard himself secured and measured photographs of his four clusters and continued his survey of them by their aid. This important work of his, begun thirty years ago, still awaits publication.

This was the first time in which the two methods of measurement had been thus compared, and indeed I know of no other instance since then in which this has been done so directly, the measures being made nearly simultaneously upon the same objects with the same telescope. But during the past twenty-five years we have ample evidence of other kinds that the result does not apply merely to this telescope but that it is universal, and that for the class of measurement here concerned, photographs obtained with long-focus telescopes, visual or photographic, give results several times as accurate as micrometer measures made with telescopes of the same size. It is very instructive to inquire what the reason is for the superiority of the photographs. The first explanation that occurs to us concerns the relative comfort in which the two operations are carried out. If the object is near the zenith it is by no means easy to set micrometer wires accurately, with the head thrown back into a nearly horizontal position and with both arms held high above. In winter, when arms and shoulders are encumbered with heavy clothing, it is especially difficult to maintain a high degree of accuracy in making such settings. On the other hand, the photographic observer, though he suffers perhaps the same degree of discomfort when he exposes his plates, makes the actual measurement upon them under ideal circumstances, in a comfortably heated room and looking down upon the plate at a comfortable angle. But after all, this explanation can account at most for only a small part of the discrepancy. Those who have used the micrometer extensively maintain that the only effect of observing under unfavourable circumstances is to make progress at the telescope slower than it would otherwise be, and they are not willing to admit that the accuracy of their measures is much affected. An examination of their work bears them out in this contention, for if this explanation were the true one we should expect winter observations made on objects nearly overhead to be decidedly inferior to summer observations made at greater zenith distances, but the published data show only a slight difference.

What I believe is the correct explanation of the superiority of photographic measures is furnished by a refraction phenomenon that has recently come to light. Some years ago Nusl and Fric, working at Prague with an instrument somewhat similar in principle to the Prism Astrolabe

that has found much favour in France, concluded from observations on Polaris that there were present at their station irregularities in refraction that caused this star to appear alternately above and below its average height for relatively long intervals. I was able to confirm the presence of such oscillations by means of photographs secured at the Yerkes Observatory, at Allegheny, and at Mount Wilson. From these it appears that at all these places changes in refraction are always or nearly always present even for objects near the zenith; they have a period that is roughly of the order of a minute of time, and an amplitude that is about a second of arc. Such fluctuations are not to be confounded with unsteady seeing; they are very much slower and seem to be present to about the same extent whether the seeing is good or bad. Fig. 1 shows their character. It is based upon measurements of a trail of Alcyone on a plate exposed in the Yerkes refractor, the total length of this trail being 140 millimetres and corresponding to three minutes of time. Each point in the figure represents the average position of the trail during about three seconds of time. It will be

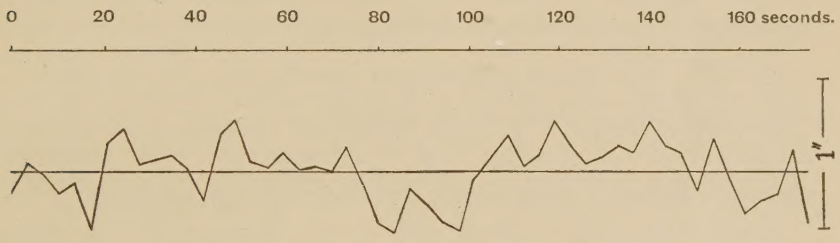

Fig. 1.—Irregularities in Atmospheric Refraction.

seen that the trail fluctuates slowly around its mean position. In one case, for example, near the centre of the figure, it remained well below for half a minute; and this excursion was immediately followed by one to the opposite side lasting nearly a minute. The greatest departure of this particular trail from the mean position is 0″·4.

What correlation exists between these fluctuations for two stars in the same field? This question is a very important one to the photographer. Fig. 2 represents the results of measuring trails of Merope (the upper line) and of Alcyone (the lower) upon the same plate. These two stars are a little less than twenty minutes of arc apart in the sky. They are so placed in the diagram that points corresponding in time are in the same vertical line. The greatest departure of the Merope trail from its mean position corresponds to 0″·5. As Merope precedes Alcyone by more than a minute of time, the last part of its trail is necessarily missing on a diagram of this kind.

It is seen that these fluctuations in refractions have just the character that we would wish them to have. They follow almost precisely the same course; and the coincidence is really closer than this diagram indicates, since the small outstanding differences can be fully accounted for as accidental errors that inevitably beset measurements of so difficult an object as a trail.

Such pairs of trails, of which I have examined many, taken under a great variety of conditions, always exhibit this same desirable characteristic ; they show that the relative position of any two stars in the same field is left unchanged by these fluctuations in the refraction, though each star usually oscillates about its average position through a range of about one second of arc. It follows that if distances, or relative rectangular co-ordinates, are measured on a photographic plate we get results that are not affected by these fluctuations. The case is in general different for measurements with the filar micrometer ; the fixed wire having been carefully placed upon one of the stars, the observer turns his attention to the other star and places the movable wire upon it, but in the meantime the kind of change in refraction that we are discussing may have intervened, in which case the measurement will be vitiated by the amount of the change. These changes are of the right order to account in this way for the disparity in accuracy between photographic and visual measures with long-focus instruments.

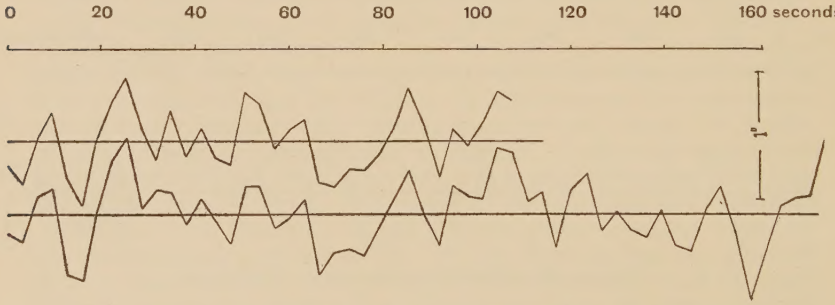

Fig. 2.—Simultaneous Trails of Merope and Alcyone.

It may be remarked in passing that the accuracy of micrometer measures of double stars does not suffer from this effect. In this case the observer can turn his attention so rapidly from one object to the other that the settings are substantially simultaneous ; indeed such measures, instead of necessitating the actual setting of micrometer wires upon the two objects, consists more in comparing their separation with the interval between the wires, this interval being adjusted until equality is reached. As soon, however, as the separation becomes too large to permit this, the accuracy falls off.

It is not my purpose to devote the short time at my disposal this afternoon to anything like a survey of our subject. I must content myself with calling your attention to two or three special problems with which photography seems especially fitted to deal. Let us begin with trigonometrical determinations of stellar parallax. Upon this problem has been tried every considerable improvement in instruments or methods made during the past three centuries. So distant are the stars that the quantities to be measured even in telescopes of great focal length are extremely small, seldom reaching the ten-thousandth part of an inch. But this is not the chief difficulty. Among the constants

with which astronomy is concerned, we may distinguish as especially difficult of evaluation those that apply to phenomena having annual periods. In these cases our real task consists in disentangling the particular phenomenon we are interested in from others of this period, and more especially from merely incidental effects arising from changes in the season. Among the latter are changes in the focal length of our instruments, in the average character of the seeing, in the average refraction, in the direction of the wind, and so on almost indefinitely. This explains why, for example, we know so little concerning the so-called *z*-term, or Kimura effect, in latitude variations, and why we are still in doubt as to whether this phenomenon may not be wholly spurious. This is why the constant of aberration is so uncertain and that values that vary from 20″·47 to 20″·53 appear in very recent work. And this is why the problem of determining stellar parallaxes has deservedly gained its reputation as among the most exasperating with which physical science is forced to deal.

The history of the measurement of stellar parallaxes has presented, more than any other department of astronomy, a continual struggle between the necessities of the problem and the methods for attacking it, very similar to the conflict that has gone on between heavier and heavier artillery and stronger and stronger armour-plate. A source of error having once been revealed, it is seldom that much time has elapsed until methods for eliminating it or avoiding it have been devised. After such improvements have been applied, new but smaller sources of error come to light to challenge our patience and ingenuity.

As the result of efforts embracing a score of years and half a score of telescopes, photography now enables us to determine from a series of fifteen to twenty plates, parallaxes whose true probable errors are 0″·008 or lower, and that are free, under favourable circumstances, from systematic error to the extent of 0″·001 or at most 0″·002.

This is not the occasion for going into details concerning the precautions that must be taken to obtain results of this kind, and I shall not attempt to do so ; it will, however, be instructive, and I hope not uninteresting, to consider one such matter. During the exposure of a parallax plate (varying from half a minute to half an hour for different telescopes) all the star images that fall upon it describe complicated paths, chiefly because of unsteadiness of the images even when the seeing is good. The left-hand curve in fig. 3 is supposed to represent such a path during perhaps two or three seconds. As in the case of the slow oscillations in refraction that we have already discussed, we have convincing evidence that this path is very precisely the same for all the stars on the plate. Nevertheless they are not registered alike for all stars ; on account of the inertia of the sensitive film, rapid excursions have little or no effect for faint stars, but they are fully registered for bright ones. This and other causes give rise to guiding errors that may reach several tenths of a second of arc, or many times the accidental error to which the measurements themselves are liable. This is particularly serious in the present connection, because the star whose parallax we wish to determine is usually much brighter than the comparison stars in its vicinity to which we refer it.

The device now universally employed to avoid guiding error is a rotating disk mounted just in front of the plate. It has a sector cut out of it whose angle can be varied. As this is turned rapidly in front of the parallax star it permits merely samples of its path to be recorded on the plates, while the complete paths of the comparison stars are registered, since they lie beyond the small central circle that the disk occults. The effect is illustrated at the right-hand of fig. 3, where the short curves represent the samples that we should get out of the left-hand curve if the sector were opened to 36° or one-tenth of a circumference. The parallax star is thus reduced nearly 2·5 magnitudes. Experience has shown that this device eliminates the guiding error very completely.

I have said that the sector is universally employed, but there is one

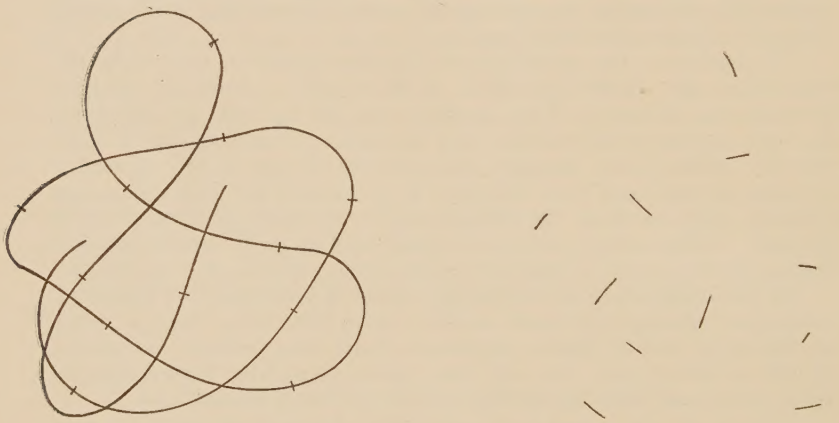

Fig. 3.—The Function of the Rotating Disk.

exception to this. At Dearborn Observatory, Professor Fox has carried out a long series of parallax determinations on faint stars ; they are in some cases as faint as the comparison stars, but on the average they are about a magnitude brighter. Under these circumstances no great guiding error is to be expected, and Professor Fox deliberately eschewed the use of the rotating sector to see how his results would be affected, if at all. The outcome of this extremely laborious research is very surprising and correspondingly valuable. Fig. 4, which I am enabled to show here through Professor Fox's kindness, gives the result of comparing his parallaxes with those obtained at other observatories, the rotating sector having been used at all of the latter. Each comparison is plotted in the figure, the ordinate being the amount by which the Dearborn parallax exceeds the other, the abscissa being the difference in magnitude between each parallax star and the mean of the comparison stars in the same field. It will be seen that for the few cases (at the left of the figure) for which the parallax star is fainter, the Dearborn parallax is smaller than those derived elsewhere ; and

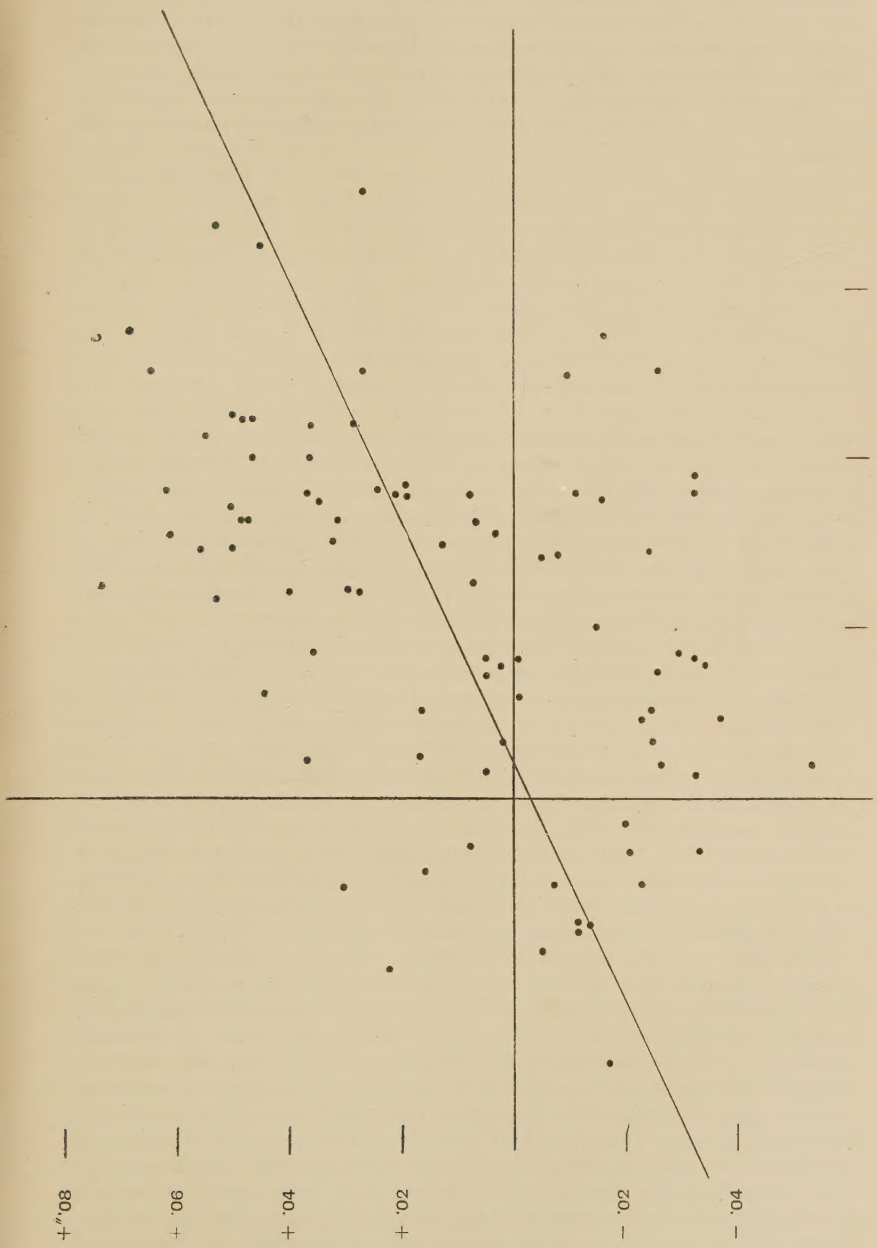

Fig. 4.—Systematic Error depending upon Magnitude.

that when the parallax star is brighter than the comparison stars (on the right of the figure) the Dearborn parallax is larger. The inclined straight line represents a least-squares solution of the data ; it indicates that a difference of one magnitude introduces a systematic error of 0″·030, a very large quantity. If the Dearborn parallaxes are compared with each of the other series separately, this coefficient comes out as follows :—

		Number of comparisons.
Dearborn–Allegheny	+0″·036	21
„ –M'Cormick	+ ·027	31
„ –Yerkes	+ ·037	33
„ –Sproul	+ ·033	6
„ –Mt. Wilson	+ ·028	8
„ –Greenwich	+ ·003	9

These coefficients all have the same sign and (except in one case for which the data are rather meagre) they are of about the same size. It appears, then, that the rotating sector not only reduces accidental error, but that it (or some similar device) is essential from the point of view of systematic error. Without it the derived parallax is much in error even if the star under investigation is only slightly brighter than its comparison stars. These results indicate that we must match the comparison stars within at most a tenth of a magnitude.

It is not easy to explain this systematic error, and one of the reasons that have led me to describe the circumstances in some detail this afternoon is the hope that some of you may be able to suggest an adequate explanation. To be sure, it is not unlikely that the guiding error at any one station is systematically different in summer and in winter. This would introduce an error in the parallaxes, but not of the kind here revealed. If the guiding error is different in direction or amount at opposite seasons, then some parallaxes will come out fictitiously large while others will be too small. To account in this way for a general increase we should have to suppose that the guiding error has the same character on the evenings of both summer and winter, and of the opposite character on summer and winter mornings.

We pass on to a consideration of the rôle that photography has played and can play in the determination of proper motions. This is a very different problem from that presented by stellar parallaxes, and a far less difficult one. Given two observations of a star at different epochs, a good determination of the proper motion can be made no matter how inaccurate the observations are, if only the interval between them is great enough. It has often been said that astronomy is a branch of physics. I do not think that this is true except in a somewhat narrow sense, although I would not go as far as does a colleague of mine in maintaining that physics is a branch of astronomy. Certainly the work that the two sciences demand may be very different in character. Physicists (and chemists too) are accustomed to begin and complete their own experiments. The astronomer, on the other hand,

is continually referring to the work of his predecessors and repeating it in order to see what changes time has brought about; and he is continually making observations that can be of no use in his own lifetime, but he expects that his successors will be glad to use them in their day. I do not know of any case in which a physicist has begun or completed an experiment of another generation. If he is interested in earlier observations at all it is only in the historical sense. I would remind you that Halley determined the first proper motions by comparing observations made in the seventeenth century with some made by Hipparchus and others at least eighteen centuries earlier; and that the wonderfully accurate work of Bradley, carried out nearly two centuries ago, still plays an important part in this subject. Equally striking examples might be drawn from other departments of our science, from any in fact in which the phenomena concerned unfold themselves very slowly. Though the inability to complete his own experiments is often a sore trial to the astronomer, he has the compensation of knowing that painstaking and honest observations are likely to survive as useful data for long periods, even though they may soon be excelled in accuracy.

Nearly half a century ago our predecessors set down in the *Astronomische Gesellschaft Catalogues* the places of about 130,000 stars in the northern hemisphere, with as great an accuracy as was feasible with so extensive an observing list. This work was, of course, all done by means of the meridian circle. A few years ago we began to realize that the time was approaching for us to repeat this work and so derive the proper motions of this vast number of stars. It was very natural that the practicability of dealing by photography with this repetition of the Gesellschaft catalogues should occur to us. At Allegheny Observatory and later at Yale we tried wide-angle fields (5° by 5°) for this purpose and found that they gave unexpectedly good results. From two observations, one image on each of two plates, we were able to obtain positions with a probable error of 0″·16. This is less than half that of the average of the original visual work in which, moreover, three observations were secured for each star. Even more gratifying is the economy of the photographic method. The number of hours spent at the telescope is reduced by a factor of ten or more, and the remainder of the process is so expeditious as to bring the reobservation of all our 130,000 stars well within the powers of a single institution instead of requiring the co-operation of more than a dozen.

We have published the repetition by photography of two of the Gesellschaft zones, and a third is well on toward completion. In the meantime the *Astronomische Gesellschaft* itself has recently decided to reobserve these stars and has adopted in all essential details the methods that we are employing at Yale. At first sight this may seem a needless duplication of effort, but I think a closer study of the circumstances will serve to convince us that it will be well to have these two undertakings go on side by side. The problems that demand an extension of our knowledge of proper motions may be divided into two classes: those for which a large number of fairly accurate proper motions will suffice, and those that require proper motions of the

highest accuracy we can attain. Of the former we have accumulated many thousands, chiefly during the last decade, through the work done at Greenwich, Yale, Babelsberg, Lund, and elsewhere. The number in the second class naturally depends upon our definition of what constitutes an accurate proper motion; if we accept probable errors not greater than 0″·004 the number is less than 3000.

It is easily within our grasp to increase the number of fairly accurate proper motions. A simple comparison of a good place at this date with the one in the Gesellschaft zones will give a result whose probable error will average about 0″·008. From this point of view two repetitions are unnecessary: a comparison of the mean of *two* modern places with the Gesellschaft place will reduce this probable error by only 4 per cent., the reason being that the modern place is so much more accurate than the old that almost all the uncertainty in the proper motion arises from the uncertainty in the earlier place.

The case is very different for very accurate proper motions. A full century must be allowed to elapse after the date of the original Gesellschaft observations in order to obtain from them proper motions whose average probable errors will be 0″·004, and two centuries if we wish to attain 0″·002 in this way. There can be no doubt that information as accurate as this will soon be indispensable to progress in the determination of the arrangement and the motions of stars. From this point of view we can hardly err on the side of doing too much in the effort to set down the positions of many faint stars at this epoch with the highest accuracy, especially in view of the ease with which photography now enables us to do such work.

Considerations like these must have been in mind when the *Astronomische Gesellschaft* decided a few months ago to carry out complete reobservations of these stars side by side with the work already under way at Yale. The two plans will be independent and should serve to check each other in a way that will surely earn the gratitude of those who will have use for them in the future. From the same point of view it would have been better if the details of the two plans differed more than they actually do; the character of the objective, its focal length, the size of the field employed, the number of comparison stars for a plate, indeed all the essential circumstances are much the same. In addition to our work with fields embracing twenty-five square degrees, we have been experimenting with much larger fields (up to 150 square degrees), with the help of a lens designed by Dr. Frank E. Ross. We have found that these plates yield positions of about the same degree of accuracy as the smaller ones. Whether they can be as easily and as completely freed from systematic error remains to be seen; this can only be decided by carrying out a large piece of work with such plates. Accordingly, Dr. Barney and I are about to measure in this way the two Gesellschaft zones that extend from 20° to 30° north declination. If the outcome should be successful we shall continue our work with these large fields, chiefly in order that the Gesellschaft plan and ours may exhibit as great a difference in details as is feasible.

As an example of the many applications for which accurate proper

motions are and will be demanded, we have in fig. 5 a diagram of the proper motions of all the Gesellschaft stars from right ascension $17^h\ 0^m$ to $17^h\ 30^m$ and from declination 50° to 55°. This is one of forty-eight diagrams constructed in a study at Yale of the proper motions in this zone. The distance and direction of each dot from the origin represents a proper motion, so that stars having small motions have their dots near the centre, those whose annual motions are 0″·1 are on the circle, with still larger motions outside of it. The directions of the two star-streams or preferential motions are shown by the arrows. The only feature that I wish to call attention to now is the existence

Fig. 5.—Proper Motions of Faint Stars.

of pairs and small groups of stars of common proper motion, of which you will see several cases on this diagram, and on almost every one of the other forty-seven like it in the other right ascensions. Fig. 6 is the most striking case of this kind that I have found in this zone. It indicates the position and motion of every star whose proper motion exceeds 0″·1 within the area $18^h\ 0^m$ to $18^h\ 30^m$. There are thirty-two such stars in this region. It will be noticed that no less than thirteen of them are equal in amount and direction within the limit indicated by the probable errors of these motions. The coincidence is all the more striking because in the two adjacent regions in this zone, each of the same area as the one in the figure, there is only one star whose motion appears to be equal and parallel to these thirteen. It is clear that we have here a moving cluster like the one in Taurus. Doubtless some

of these coincidences (on both figs. 5 and 6) are accidental, and doubtless also we have missed some members of these groups for the same reason, accidental error apparently carrying them outside the limits we set for equality. Such questions cannot be decided until we have more accurate proper motions, nor until then can we locate the convergent of these motions on the celestial sphere. This example, as I have already said, is the most striking of its kind that this zone presents, but it is by no means the only one.

FIG. 6.—Proper Motions of Faint Stars.

In the case of clusters like the Pleiades and Præsepe our attention is called to them by the mere grouping or crowding of the stars as seen with the naked eye or on photographic plates. In such cases we can decide which stars belong to the cluster and which do not, by means of their proper motions even if these be small. It is different for a cluster like the one in Taurus or the one in fig. 6 ; the cluster is revealed through a study of proper motions, and to detect them the motions must be large in comparison with their probable errors. In the present state of our knowledge there are very few cases in which we can hope to find such a cluster unless the common motion is nearly as much as $0''.1$ per annum. When at some future day we have at hand accurate proper motions in much greater numbers than our catalogues now

contain, I have no doubt that many such clusters will be found. Unless the conditions in this particular zone are exceptional (it is hard to see why they should be so, and in fact there is evidence to the contrary which I cannot stop to recall now) the indications are that a large fraction of all the stars in the sky as bright as the ninth magnitude will be found to be members of moving clusters. Some of these clusters may embrace only a few stars, while others may be found to contain hundreds. In my opinion the problems concerning moving clusters present by far the strongest incentive for us to lay now, truly and well, the foundation for the determination of many accurate proper motions.

To turn to another phase of our subject, it is well understood how nearly free from personal equation photographic measurements can be made; but here again this important characteristic was certainly not fully recognized at the close of the nineteenth century. When in 1899 the International Geodetic Association established its chain of latitude stations on the 39th parallel, the visual zenith telescope was chosen as the standard instrument with which to determine motions of the terrestrial pole, though advocates for a photographic instrument were not lacking at that time. Owing to a wise reluctance to make changes in such work as this, the three surviving stations have continued to use these visual instruments through all these years. It is not unlikely that the near future will see the establishment of other stations, especially in latitudes very different from that of the 39th north parallel. With a view to assisting in such a project we have been experimenting at Yale with a photographic latitude instrument, the construction of which is shown in fig. 7. These observations have been carried out chiefly with the able assistance of Mr. Oort, now of the Leiden Observatory. A horizontal 10-inch objective of 100 inches focal length has a narrow photographic plate fastened to its rear surface, with its film down. Upon this, zenith stars leave trails as they pass through the meridian, their light being reflected to the plate from a mercury surface about 50 inches below. This device is of course not particularly new; its essential features will be found in the work of Airy, Kapteyn, Cookson, and Ross; the only new feature in our use of it is its entirely automatic character. It is necessary to reverse the objective several times during the night—that is, to turn it through a half circumference around a vertical axis. It is also necessary to uncover the objective only when a star is trailing, for if it is left uncovered during several hours the light of the sky will fog the plate, the ratio of the aperture to the focal length being so large. Both of these operations are carried out faithfully by an old sidereal clock, to the hour-arbor of which we have attached two large aluminium rings, and upon the perimeter of these we have fixed adjustable electric contacts that close circuits at the same sidereal time every night. These circuits operate two sets of magnets, so arranged as to carry out the two necessary operations described above. All that the observer need do is to insert a plate after dark and remove it before dawn, in the meantime devoting his energies to other observing. The instrument should therefore be well adapted for operation at fixed observatories where other work is going on at night. Mechanically the instrument works very well. Just how

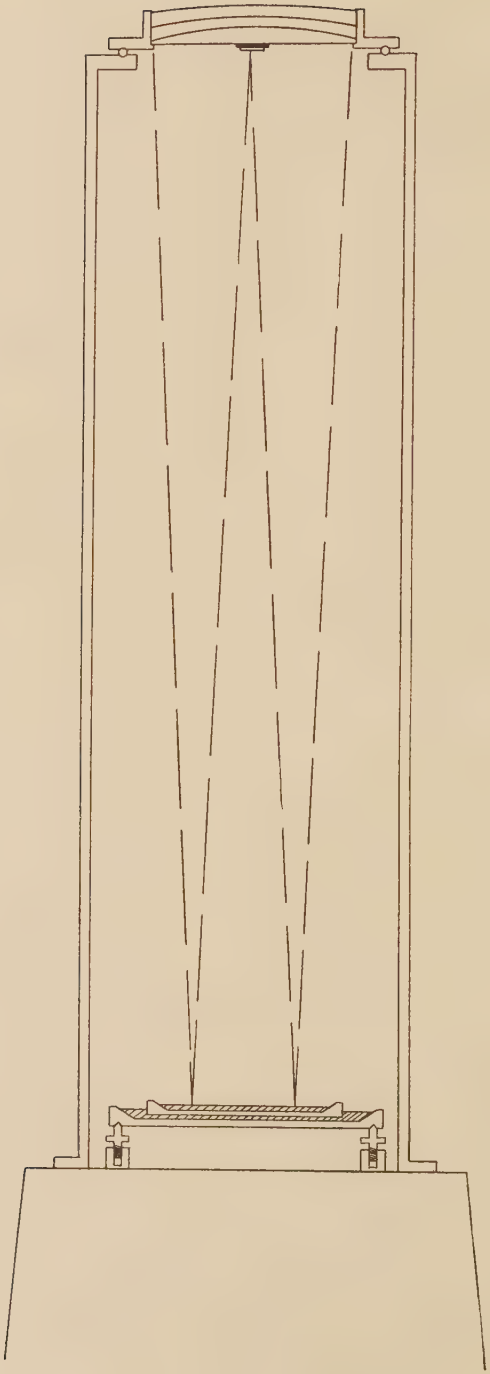

Fig. 7.—Zenith Camera.

accurately the latitude can be determined with it, we do not yet know, but we have carried the measurements and reductions to the stage that permits us to say that the results will compare favourably with those derived with other instruments, especially in view of the comparatively short focal length which is a necessary feature if we are to secure trails of faint stars. No one is near the instrument while it is observing, and this is an advantage. The wooden shelter around it is unusually well ventilated ; and there is no telescope tube, the objective being mounted on an open support made up of angle-iron. These circumstances will, we hope, help to eliminate the small but important systematic errors to which latitude observations seem to be liable.

Let me terminate this lecture with a brief description of the refracting telescope that we set up at Johannesburg in 1925 as a southern branch of Yale Observatory. This instrument was designed especially for astrometric work by means of photography. A disadvantage of the ordinary mountings for refracting telescopes when they are used for photography is that their piers interfere with exposures in the most important part of the sky, namely, near the meridian from about 20° south of the zenith to about 20° north. Thus, for example, the plate-end of the Yerkes Refractor strikes the pier when objects in certain declinations have passed the meridian by as little as eight minutes. When the Allegheny Refractor was designed we partly cured this inconvenience by making the declination axis longer and the pier narrower in an east and west direction, so that this telescope does not strike until the meridian is passed by at least half an hour. To avoid this trouble completely a radically different form of mounting is necessary. When the time came to design our southern telescope we first made drawings for the so-called English mounting, in which the polar axis is in the form of a long cradle supported at both ends, with the telescope tube slung between its two sides. This we rejected because of the difficulty of making the cradle rigid enough without resorting to very heavy members, and also because it is impracticable with this type of mounting to permit pointings on the pole without making the polar axis very long or else encountering some equally objectionable feature.

The form of mounting adopted is shown in fig. 8. The part of the polar axis below the centre of motion is long enough to permit the plate-end of the telescope to swing past the worm wheel at the lower end, thus enabling us to point at the pole and to reverse the telescope. The piers cannot interfere with exposures until the telescope is pointed beyond about 20° north declination, a position that is demanded in none of the regular work contemplated. The telescope tube is mounted as close to the polar axis as possible. This tube, 36 feet long, is made entirely of a light but very strong alloy of aluminium, as is the cell for the 26-inch objective ; but all the other parts are made of cast steel or iron. To avoid the use of mitre gears the driving-clock is mounted on the massive wall of the observatory, and is so connected with the driving worm that the two may move in any direction with respect to each other without cramping and without introducing appreciable irregularities in the driving.

The work planned for this telescope is astrometric. Such observations can best be made near the meridian, and in fact a good many of them must be made there. For this reason we have not thought it necessary to mount this telescope in the ordinary dome, but have instead sheltered it by the far less expensive structure indicated in fig. 8. The ground plan is rectangular; 47 feet in the north and south direction, 18 feet

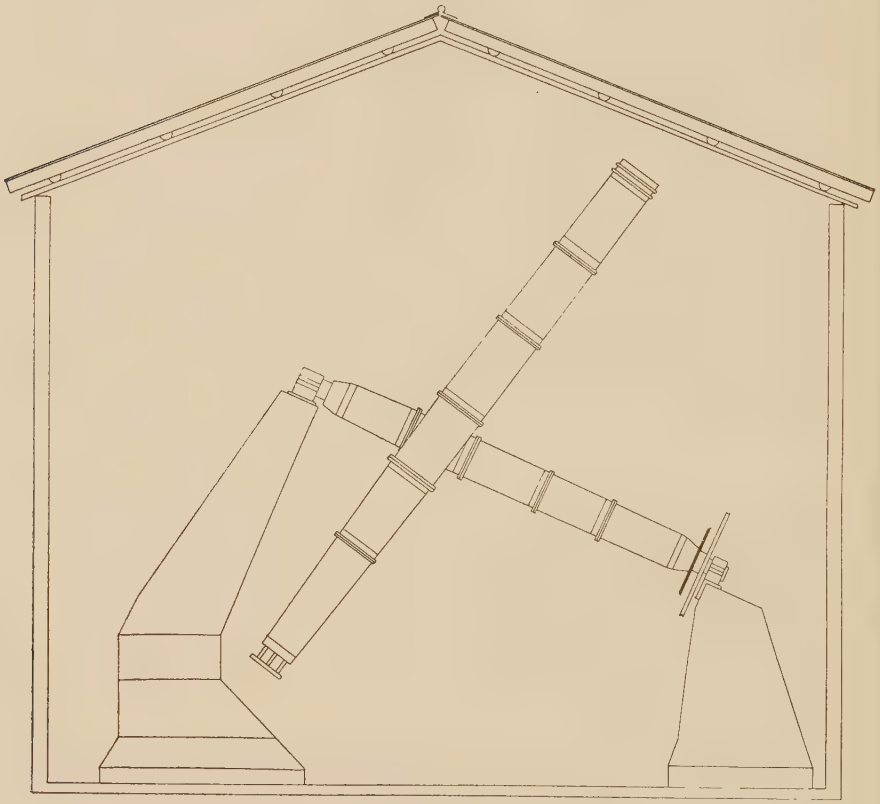

FIG. 8.—Yale Telescope at Johannesburg.

east and west. To permit access to the sky the roof rolls off in two halves, one to the north and one to the south, leaving clear all that part of the sky in which we are proposing to observe. In addition there are two sets of large doors at the tops of the north and south walls, which when opened permit observations to be made at still greater zenith distances, so that only objects north of about 45° north declination do not come into view on the meridian.

Of course the disadvantage of this type of observatory is the restriction of the observations to the neighbourhood of the meridian. In addition to its inexpensiveness, this kind of shelter has the advantage

of offering very complete protection against the wind. The east and west walls come well beyond the telescope tube in all its positions ; and if there is a strong wind from the north or south the observer may close, partly or entirely, the corresponding half of the roof and then confine his work for that evening to the other half of the sky. But a far more important advantage is with respect to ventilation. At least half an hour before dark the roof is opened to its limit ; this leaves uncovered the whole of the observatory below, and permits the very rapid escape of the warmer air that has accumulated during the day. There is reason to believe that good seeing is largely a matter of good ventilation, and it is not at all improbable that some sites which would otherwise have proved good ones with regard to seeing have been spoiled through a neglect of this principle. Our observatory is built in the shade of two rows of tall gum trees to the west, so that it is protected from the heat of the sun during the hours just preceding work. There are three windows in the east wall, but none in the west wall or on the north or south, so that only in the morning can sunshine reach the interior. Finally, the walls are double with an air-space between the two parts, brick on the inside and artificial stone on the outside. There are many vents in the outer wall connecting with the air-space between, so that as the air becomes warm it escapes to the outside, leaving the interior of the observatory cool. But perhaps the most important precaution of this kind that we have taken is in the telescope tube itself. Just below the objective cell are two large arm-holes opposite each other. Incidentally these permit access to the rear surface of the objective for cleaning and other purposes, but their chief function is to ventilate the tube. In front of one of them is mounted an electric fan. This is turned on a half-hour or so before beginning to observe in the evening. It blows a steady stream of air into this opening, and as the plate-end of the telescope below is also open, the whole tube is emptied of warm air and soon acquires the temperature that prevails outside the tube and outside the observatory. This fan is turned on at frequent intervals during the night ; it is on the same circuit as the lights that permit us to read the right ascension and the declination circles, and so the fan operates for about two minutes whenever the observer points to a new object, which in most of our work occurs every seven or eight minutes. Experience with this device shows that it actually does improve the character of the images, and that, moreover, it obviates changes in focus that may otherwise be very annoying. If these precautions are not as effective as we hope, it is at least a satisfaction to know that we have done our best not to spoil the quality of starlight in the last few feet of so vast a journey.

On Dr. Fotheringham's Paper entitled "Trepidation."

By Ernest W. Brown.

In his paper * "Trepidation," Dr. Fotheringham deduces from the observations of the Sun an apparent change of 1 or 2 per cent. in the ratio of the mass of Venus to that of the Sun coincident with the major changes in the apparent fluctuations of the longitude of the Moon. He attributes this to a real change taking place in this ratio. I propose to show that any such change is highly improbable on account of other effects which it would produce in the motion of the Sun. The latter part of my paper contains reasons for the suggestion that the correlation is accidental, and that it may be due to different values of the coefficients of the perturbations used in comparing theory with observation.

If $\delta\epsilon$ be the observed minus tabular difference, δL, $\delta\mu'$, δe, and $e\delta\varpi$ changes in the true longitude, the ratio of the mass of Venus to that of the Sun, the eccentricity and the eccentricity multiplied by the change of the longitude of perihelion, the equation of condition given by any observation is

$$\delta L + M' . \delta\mu' + E . \delta e + \Pi . e\delta\varpi = \delta\epsilon.$$

If g and g' be the mean anomalies of the Earth and Venus, we have approximately

$$E = 2 \sin g, \qquad \Pi = -2 \cos g,$$

and †

$$\begin{aligned} M = {} & 4''{\cdot}84 \cos (g - g' - 299^\circ) + 5''{\cdot}53 \cos (2g - 2g' - 148^\circ) \\ & + 2''{\cdot}50 \cos (3g - 2g' - 316^\circ) + 1''{\cdot}88 \sin (13g - 8g' + 223^\circ) \\ & + \text{smaller terms.} \end{aligned}$$

Newcomb solves the equations of condition in groups of about eight years. Over each group the term with argument $13g - 8g'$ (having a period of about 240 years) remains substantially constant. The other terms pass through a number of complete revolutions sufficient to avoid errors due to parts of the terms only being present. In each group the equations were solved by least squares and the various changes obtained separately.

If $\delta\epsilon$ contain a term with very long period, it is easily seen from the numerical equations for any group that its effect is almost wholly confined to δL, and that it exerts but little influence on the value of $\delta\mu'$. Similarly, any terms with period equal to, or nearly equal to, a year only affect δe and $e\delta\varpi$. To affect $\delta\mu'$ sensibly it is necessary to have changes in $\delta\epsilon$ with periods nearly equal to the periods of $g - g'$ or of $2g - 2g'$, that is to say, to periods of nineteen months and ten months respectively. Hence, if a change in $\delta\mu'$ appears after we compare the solutions of the various groups, and if to be attributed to an

* *M.N.*, **87**, 142 (1926).
† Newcomb, *Tables of the Sun.*

actual change in μ' of a periodic character this change must have a period nearly equal to that of $g - g'$ or to that of $2g - 2g'$. Suppose the hypothesis be adopted that the period is nearly equal to that of $g - g'$ and that it be expressed by the term

$$A \sin (g - g' + \alpha t + \beta).$$

It is a simple matter to show that a long-period term in the longitude of the Sun will be produced with an approximate coefficient of

$$\tfrac{1}{2}4'' \cdot 8(p/\alpha)^2 A,$$

where p is the motion of the argument $g - g'$.

Dr. Fotheringham's hypothesis demands that the period of $\alpha t + \beta$ be of the order of 200 years or more, and, further, that A be 0·01 at least. These numbers give a coefficient of 375″.

If we adopt a similar hypothesis for the term with argument $2g - 2g'$ we get an even larger coefficient. The hypothesis therefore appears to be untenable.

While Dr. Fotheringham's assumption of a periodic change of mass cannot hold for gravitational reasons, an explanation of his correlation between the determinations of the mass of Venus and the apparent fluctuations in the Moon's motion is needed. My suggestion is that the correlation is accidental and is due to the use of different values of the coefficients of the principal Venus terms used in the tabular material. This latter appears, at the first glance, to be divided roughly into the same three periods as the major changes in the fluctuations, namely, the latter half of the eighteenth century, the nineteenth century, and the first quarter of the twentieth century; but inspection shows that the chief break from the first to the second period is rather at 1830 than near 1800.

If we solve the nine groups given by Dr. Fotheringham on p. 162 of his paper on this basis, the solution will be seen to fit the observations nearly as well as his. In fact the chief change is not numerical, but is in the meaning which is to be attributed to his ϕ. He makes this quantity mean the rate of change of the lunar fluctuation; I attribute it to the tabular values adopted.

In justification of this latter suggestion I quote * certain paragraphs from pp. 6, 7, and 8 of Newcomb's *Astronomical Constants*: "As the tables in their original form are extremely cumbrous to use, the theory was partially reconstructed by making manuscript tables of the principal perturbations, which were, however, *carried only to tenths of seconds*. With these tables the places of the planets were computed for *dates previous to* 1864. . . ."

"The method of deriving the final tabular places varied with circumstances. When there was no accurate ephemeris available for comparison, *which was the case before* 1830, it was necessary to compute a completely independent set of tabular geocentric places. Sometimes these places were computed for the moment of the individual observations, but more generally, when the observations occurred in groups,

* The italics in these quotations are mine.

an ephemeris was computed in order that the work might be checked by differences. . . ."

" Until this plan was mapped out and work well in progress upon it, it was not noticed that the planetary masses adopted in Leverrier's tables were so diverse that corrections to reduce the geocentric places to a uniform system of masses would be necessary. Although theoretically the necessary reductions were very simple, I cannot but feel that *the application of such corrections involves more or less doubt and uncertainty*, and that it would have been better to have constructed provisional tables based on uniform masses quite independent of those of Leverrier. . . ."

" As in the actual work the masses of Mercury and Venus were to be determined from the observed periodic perturbations which they produced, it was necessary that the perturbations produced by them *should all be carefully reduced to the adopted standard.* The reduction was less necessary in the case of Mars, but was carried through all the work relating to the Sun."

Now the differences on which Dr. Fotheringham bases his hypothesis depend on the second places of decimals in the tabular coefficients, expressed in seconds of arc. Let us therefore consider the statement in the first quotation. If the second place was cut off before tabulation, the coefficient to be used for comparison is the truncated value ; if after tabulation but before summation, it may be less than the complete value, because when the second figure is less than 5, the complete value is never reached in the tabulation. It is only when it is cut off after the summation that we can be fairly confident that the complete value of the coefficient is implicitly present in the ephemeris. Now the last is the method ordinarily adopted in tables of perturbations, and Newcomb would not have made the statement in the first quotation if he had adopted the usual procedure. As we do not have the details of his work we do not know whether the second place was cut off before tabulation or after, or what figures were cut off when the two principal terms were truncated. In fact the statement in the last quotation is not quite consistent with that in the first. I am inclined to think that the cutting off was done before tabulation, because the chief saving of labour would be gained in that way. Additional systematic errors would be caused also by the omission of terms.

There is therefore nothing surprising in the discovery of a value of $\delta\mu'$ different after 1864 from that before. The second quotation further shows that there was also a different procedure before and after 1830. What it really means with reference to the coefficients under consideration is not clear. Finally, we probably have a new set of coefficients at the end of the nineteenth century, when Newcomb's tables were replacing those of Leverrier. We might perhaps have felt confident that Newcomb had taken care of all the changes and reduced them to a uniform system if, as shown in the third quotation, he had not himself cast doubts on the accuracy of this part of his work.

We have thus at least three, and possibly four, different periods as far as the tabular material is concerned. Now if we plot the values of $\delta\mu'$ which he deduces, for example, from the long Greenwich series

(1750–1892), it is at once seen that a definite break occurs about 1830. Before that date a mean value of $-\cdot03$ is indicated; after it, one of $+\cdot01$ or $+\cdot02$. And if we look at the dispersion, it is evident that the material scarcely justifies the formation of means over shorter intervals, and the computation of probable errors is misleading where systematic errors are present.

A nearly parallel change in the values of δe, $e\delta\varpi$ also occurs, but in the case of $e\delta\varpi$ there is a marked minimum extending over twenty years near the beginning of the nineteenth century. In both cases there is a rather sudden change near 1830. After this date both are well represented by constant values to the end of the series. In any case, changes in these quantities have to be dealt with cautiously, since, as Newcomb points out, observational errors with a period of a year will be included.

Yale Observatory:
1926 *February* 26.

Addition, May 10.—From correspondence with Dr. Jackson and Dr. Fotheringham I find that there is obscurity in the remark at the beginning of the fifth paragraph: "If $\delta\epsilon$ contain a term with very long period, it is easily seen from the numerical equations for any group that its effect is almost wholly confined to δL. . . ."

To clear up this point, denote by $m\mathrm{R}_0$ the principal part of the "constant" term of the disturbing function for the action of Venus on the Earth. The principal effect on the mean longitude is obtained by integrating the equation

$$\frac{d\mathrm{L}}{dt} = -\frac{2}{na}m\frac{\partial \mathrm{R}_0}{\partial a}$$

where m is the mass of Venus. Suppose

$$m = m_0\{1 + \mathrm{A}\cos(pt + \alpha)\}.$$

The well-known developments give

$$\frac{\partial \mathrm{R}_0}{\partial a} = -\frac{1{\cdot}79}{a^2}, \qquad m_0 = 2{\cdot}5 \times 10^{-6}\ (\text{mass of Sun})$$
$$= 2{\cdot}5 \times 10^{-6}n^2a^3.$$

Whence, for the variable part of m,

$$\delta\mathrm{L} = \frac{n}{p}\,.\,\mathrm{A}\,.\,1''{\cdot}8\sin(pt + \alpha).$$

With $p = n/200$, $\mathrm{A} = \cdot02$, this gives a coefficient $7''{\cdot}2$, which is much too large to be admissible in the comparison of the tables of the Sun with the observations.

A Spectroscopic Determination of the Constant of Aberration. By H. Spencer Jones, M.A., Sc.D., H.M. Astronomer.

In the *History of the Cape Observatory* (p. lxxviii), Gill states that his first thought, when Mr. Frank McClean made his offer of a 24-inch refractor to the Cape Observatory, was, " Now here will be an opportunity to try how accurately the solar parallax can be determined with the spectroscope." Gill had previously expressed the opinion (*Observatory*, **14**, 214) that " with a 25- or 30-inch telescope, well mounted, and a spectrograph . . ., employing the largest dispersion that can be used, I believe that, by observation of the brightest stars, it will be possible to measure the velocity of the earth in its orbit so accurately that the solar parallax will be obtained in this way more correctly than by any other method."

A series of spectrograms for determining the solar parallax was commenced with the four-prism spectrograph of the Victoria telescope in 1906 February and completed in 1908. The measurement and reduction of these plates was undertaken by Halm, and the results published in *Cape Annals*, **10**, part 3. The final result for the solar parallax resulting from this investigation was $8''{\cdot}800 \pm 0''{\cdot}006$, satisfactorily confirming Gill's prediction as to the precision attainable by the spectroscopic method. The method does not seem to have been fully tested elsewhere.

For the purpose of deriving the solar parallax, it is necessary that spectra with large dispersion should be secured when the stars are near their quadratures with the Sun. The spectra of a number of stars were obtained under these conditions at the Cape in the years subsequent to 1908. The scale of the spectra is approximately 10 Ångstrom units = 1 mm.

The spectrograph was dismounted in 1926 March in order to undertake a programme of stellar parallax determinations with the Victoria telescope. The radial velocity observations are in course of preparation for publication, and in this connection an examination has been made of material suitable for the determination of the solar parallax.

Twenty-one stars were well observed near quadratures and are therefore available for the purpose. Of these, three have variable velocity, α Orionis, α Scorpii, and α Canis Minoris. The periods of the two former are short and the orbits uncertain (vide *Ap. J.*, **44**, 250, 1916). The earlier investigation was based upon seven stars and included both α Orionis and α Scorpii. In the reductions the changes of velocity of α Orionis were assumed proportional to the time and alternatively in the case of α Scorpii, the orbital variation of velocity was derived from a provisional orbit and allowed for. With the amount of material now available, it seems safer to discard these stars, as their velocity changes were not linear during the period covered by the observations.

The velocity variation of α Canis Minoris has a period of thirty-nine years and a range of only 1·2 km. per sec. During the period covered

by the observations the velocity variation is practically linear and has been allowed for on this assumption.

The plates have all been measured differentially in the Hartmann comparator, against one plate of the same star or against a plate of another star of similar type in general in the wave-length range λλ 4337 to 4529. For the purpose of the derivation of the solar parallax, the radial velocity of the reference plate need not be known.

The results for any one star are expressed in the form—

$$\text{Observed velocity} = a + \odot\frac{\Delta\pi}{\pi},$$

a being a constant, $\odot$ the adopted correction to the Sun, and $\Delta\pi$ the correction to the assumed value of the solar parallax (8″·800).

The computation of the corrections to the Sun have been based on the following data :—

Length of the sidereal year in seconds of mean time .	= 31558150
Equatorial radius of Earth in km. (Bessel) . . .	= 6377·397
Velocity of light in km. per second	= 299865
Length of sidereal day in seconds of mean time . .	= 86164
Sun's horizontal equatorial parallax	= 8″·800

As pointed out by Plummer (*Observatory*, **31**, 239, 1908), the spectroscopic observations actually furnish the constant of aberration, the derived value of which is independent of the assumed velocity of light and equatorial radius of the Earth. The constant of aberration is derived from the solar parallax, on the basis of the data given above, by means of the relationship—

$$k \,.\, \pi = [2{\cdot}255698].$$

Improved values for the radius of the Earth and the velocity of light are as follows :—

Equatorial radius of the Earth in km. (Hayford, 1909) * 6378·388.

Velocity of light in km. per second (Michelson, 1926)† 299800; with these values we have—

$$k \,.\, \pi = [2{\cdot}255859].$$

The value of π derived from the corrections to the Sun as computed must therefore be multiplied by [0·000161] to bring it into conformity with more modern data.

The separate results for $\Delta\pi/\pi$ are given in the following table, with the weight of each determination and the number of plates on which it is based :—

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* Recommended for adoption by the International Union of Geodesy and Geophysics.

† Mount Wilson Observatory Report (*Year-Book of the Carnegie Inst.*, 1925–6, p. 135).

TABLE I.

Star.	R.A. h m	Dec. ° ′	Type.	$\Delta\pi/\pi$.	Weight $10^4\times$.	Number of Plates.
β Ceti	0 38	−18 32	Ko	+·00166	3·10	51
α Arietis	2 1	+22 59	K2	+·00628	2·32	38
α Tauri	4 30	+16 18	K5	−·00391	4·36	61
α Can. Min.	7 34	+ 5 29	F5	+·00143	2·76	47
β Gem.	7 39	+28 16	Ko	−·00106	1·77	27
α Hydræ	9 23	− 8 13	K2	−·00385	2·20	39
ε Corvi	12 5	−22 4	Ko	−·00116	2·11	31
β Corvi	12 29	−22 51	G5	+·00070	1·97	28
ε Virg.	12 57	+11 30	Ko	+·00399	1·56	23
θ Cent.	14 0	−35 53	Ko	+·00452	2·44	37
α Bootis	14 11	+19 42	Ko	−·00196	1·87	33
α Serpentis	15 39	+ 6 44	Ko	+·00359	2·08	33
ε Scorpii	16 43	−34 7	Ko	+·00296 −·00270	2·30	30
γ Sagitt.	17 59	−30 26	Ko	−·00446 +·00012	1·77	22
δ Sagitt.	18 14	−29 52	Ko	+·00576	1·93	24
λ Sagitt.	18 21	−25 29	Ko	−·00565	1·09	16
π Sagitt.	19 3	−21 11	F2	−·00379	1·46	22
β Aquarii	21 26	− 6 1	Go	−·00339	2·21	30
α Aquarii	22 0	− 0 48	Go	+·00092	1·86	26

In the above table two values are given for $\Delta\pi/\pi$ for ϵ Scorpii and γ Sagittarii. These two stars show some evidence of variation in velocity during the period covered by the observations. ϵ Scorpii is not known to have a variable velocity, but the Lick observations of γ Sagittarii show a range in velocity of 6 km., and Wright considers the star a variable (*L.O.B.*, **6**, 153, 1911). The first value of $\Delta\pi/\pi$ given in the above table for each of these stars is derived on the supposition that the velocity is constant, and the second value is derived on the hypotheses that the velocity is variable, the variation during the period covered by the observations being proportional to the time. This period is about three years. The slow variation of α Can. Min. was assumed to be a linear function of the time.

The weighted mean value of $\Delta\pi/\pi$ is + 0·00021 or + 0·00009 according as the velocities of ϵ Scorpii and γ Sagittarii are assumed to be constant or to be variable.

The resulting values for π are—

$$8''\cdot802 \text{ or } 8''\cdot801 \text{ with probable error } \pm 0''\cdot0037.$$

In the previous series of observations, the results of which are given in *Cape Annals*, **10**, part 3, different comparison plates were

used in the measures in the Hartmann comparator. The results can be incorporated as a distinct series with their proper weight. They are as follows :—

Star.	R.A. h m	Dec. ° ′	Type.	$\Delta\pi/\pi$.	Weight $10^4\times$.	Number of Plates.
α Tauri	4 30	+16 18	K5	+·00366	2·02	25
α Orionis	5 49	+ 7 23	Mα	+·00401	2·62	35
α Can. Min.	7 34	+ 5 29	F5	−·00416	3·68	61
β Gem.	7 39	+28 16	K0	−·00189	1·66	22
α Bootis	14 11	+19 42	K0	+·00085	2·18	55
α_2 Centauri	14 32	−60 25	G0	−·00248	2·13	54
α Scorpii	16 23	−26 16	Mαp	+·00060	2·98	50

These results were derived on the assumption that the radial velocities of each star changed uniformly with the time. This assumption is legitimate for observations extending over only a short period in the case of α Can. Min. and α_2 Centauri, whose velocity change is slow. The periods of variation of α Orionis and α Scorpii are much shorter, but the diagrams given by Lunt (*Ap. J.*, **44**, 251, 256, 1916) show that the observations of both these stars fell on a portion of the velocity curve which is practically straight. The assumption is therefore substantially correct for the period under consideration.

Incorporating these results we derive the following values for the solar parallax, in which A, B denote that α Orionis and α Scorpii have been included or excluded respectively, and C, D denote that the velocities of ϵ Scorpii and γ Sagittarii have been considered as constant or variable respectively :—

1.	A, C	8″·801
2.	A, D	8·800
3.	B, C	8·799
4.	B, D	8·798.

The differences are within the probable error of the determination.

As a definitive value we adopt A, D, the solution in which the earlier results for α Orionis and α Scorpii are retained, and in which the velocities of ϵ Scorpii and γ Sagittarii have been assumed to change uniformly with the time.

The probable error of the final result, derived from the discordances of the results for individual stars is $\pm$ 0″·0029. The probable error may also be deduced from the probable error of a radial velocity determination of a single plate, $\pm$ 0·39 km./sec. The value derived in this way is $\pm$ 0·0044, somewhat larger than that derived from the discordances between the individual stars. The first series also gave a larger value of the probable error when derived from the probable error of a single plate than from the discordances amongst the mean results of the several stars. The explanation, as then suggested, may perhaps be found in the existence of short-period fluctuations in some of the

radial velocities, tending to increase the *p.e.* derived from the residuals of single plates.

Accepting the larger value of the probable error, the resulting value for the solar parallax is $8''{\cdot}800 \pm 0''{\cdot}004$. As mentioned above, this is essentially a value derived from the constant of aberration by assuming values for the velocity of light and the earth's equatorial radius, the two constants being connected by the relationship $k\pi = [2{\cdot}255698]$.

The value of the constant of aberration resulting from the Cape spectroscopic observations is therefore

$$k = 20''{\cdot}475 \pm 0''{\cdot}010.$$

Adopting Hayford's spheroid and Michelson's most recent determination of the velocity of light, the inferred value for the solar parallax is

$$\pi = 8''{\cdot}803 \pm 0''{\cdot}004.$$

The details of the measurements from which these results are derived will be published in the *Cape Annals*.

Preliminary Results Derived from the Declination Observations of the Greenwich Transit Circle, 1851–1925. By R. T. Cullen.

(Communicated by the Astronomer Royal.)

1. At the end of 1925 the Greenwich Transit Circle had been in use continuously for seventy-five years. A series of observations extending over such a period, made with one instrument in the same situation throughout, should provide reliable data for the determination of proper motions.

In this paper, the investigation has been confined to the declinations, Boss's Preliminary General Catalogue for 1900 being utilized for the purpose of comparison.

In 1896 Mr. Thackeray (*Monthly Notices*, **56**, 343–4) derived from Greenwich observations made by instruments in use from 1836 to 1893 the proper motions of a comparatively small number of bright stars, which indicated a mean correction of $+ ''{\cdot}0068$ to the annual proper motions of the original Boss system of declinations.*

About 1903, Boss revised his system, published ultimately as the "Preliminary General Catalogue of 6188 Stars for 1900."

The most important addition since, in 1925, is Eichelberger's "Positions and Proper Motions of 1504 Standard Stars for 1925·0," which contains a system of proper motions depending upon Boss positions at the mean epoch (about 1865) compared with observations made at the Cape and Washington about 1910. His results give mean corrections to the Boss proper motions in declination similar to those indicated by Mr. Thackeray.

* Boss's Declinations of 500 Stars (1875).

Eichelberger's corrections systematically affect the Boss declinations from between 0″·5 to 1″·0 per century in the neighbourhood of the equator. He found also corrections of a similar nature to the revised Newcomb and Auwers declinations.

These appear so large after such a short lapse of time that any further independent results either confirmatory or otherwise are of value.

2. With this end in view, it was considered worth while to undertake a preliminary investigation into the Greenwich declination observations confined to the period 1851 to 1925.

For this purpose the brightest clock stars were selected, ignoring Sirius, Procyon, and ζ Herculis on account of orbital motion, in all fifty-five stars representing some of the best data with continuity over the entire period.

Further, to give homogeneity to the series, only direct observations have been used, and one assumption made :—

> That these observations are not affected by any *change* of flexure in the instrument throughout its use.

Therefore, the observations as reduced have been corrected to a uniform system, viz.—

> Poulkowa refractions.
> Empirical term of + ″·60 sin Z.D. for flexure.
> Colatitude 38° 31′ 21″·85.
> Division errors now in use.

There has been much discussion in the past about some of these adopted constants, but it is not the intention here to go again into the matter, except to state that for many years these values produce good accordance between the circumpolar stars and the Sun. They are now carried right back to 1851 and applied to the whole series of observations made with the transit circle.

We may now state the advantages in favour of this method of approaching the problem :

(1) One instrument in the same position always.
(2) Observations are corrected to a uniform system and are continuous over a period of seventy-five years.

Thus the deduced proper motions are freed from uncertainties in

(*a*) refractions and division errors ;
(*b*) flexure, if assumption of no change can be assured ;
(*c*) latitude.

The method of adopting zenith points has not been uniform throughout the period, but any variations resulting from these would imply varying values of the latitude shown by the circumpolar observations. The changes are small and have been treated as of an accidental nature.

The results depend upon Poulkowa refractions and + ″·60 sin Z.D. for flexure.

Period.	Deduced Colatitude.	
1851–1857	38° 31′ 21″·93	
1860–1867	·95	
1868–1877	·92	
1878–1882	·87	
1883–1887	·76	Adopted 1851–1925.
1887–1896	·75	38° 31′ 21″·85.
1897–1905	·87	
1906–1914	·83	
1915–1921	·86	
Mean	38 31 21·86	

The values for the period 1851–1887 have been deduced with the aid of Newcomb's paper on "North Polar Distances of the Greenwich Transit Circle, 1851–1887," given in the *Astronomical Papers of the American Nautical Almanac Office* (vol. ii., part vi.).

With respect to flexure it may be mentioned here that when the R–D results showed large fluctuations these were not substantiated by the direct observations when discussed alone (see Introduction to the Greenwich Catalogue, 1900, p. 18).

3. The actual method of determination may now be outlined. Entries were made of the observed declinations for each year reduced to the beginning of the Besselian year with allowance for proper motion where necessary. Whenever further corrections to the observations were required these were applied to the mean annual declinations.

Mainly these are :

(1) Corrections to reduce from Jan. 1d·0 to beginning of Besselian year (1851–1856).
(2) Corrections for errors of microscope-micrometer screws (1868–1873).
(3) Corrections to refraction due to an erroneous thermometer reading (1872–1878).

And as various changes were made in the constants of reduction during the seventy-five years, further corrections were necessary finally to reduce the observations to the adopted constants before mentioned (section 2).

Boss's declinations for the fifty-five stars taken from his Preliminary General Catalogue, 1900, deduced annually for the years 1851–1925, were compared with the observed declinations, thus forming differences "Greenwich–Boss" for every year.

These residuals were then grouped into periods of fifteen years, namely :

1851 to 1865
1866 ,, 1880
1881 ,, 1895
1896 ,, 1910
1911 ,, 1925,

the means taken according to the number of observations and weighted proportionally.

The mean residuals may then be expressed in the form :

Greenwich–Boss.

$x-2y=\Delta\delta$ (1851–1865) with weight proportional to number of observations.
$x-1y=\Delta\delta$ (1866–1880) ,, ,, ,, ,,
$x\quad=\Delta\delta$ (1881–1895) ,, ,, ,, ,,
$x+1y=\Delta\delta$ (1896–1910) ,, ,, ,, ,,
$x+2y=\Delta\delta$ (1911–1925) ,, ,, ,, ,,

and solved by least squares, where

x represents the mean correction to the Boss declination at the mean epoch 1888·5,
and y the correction to the Boss proper motion in declination expressed as a unit of fifteen years.

Two stars have been selected to serve as a practical demonstration of the method, and to illustrate as well two widely different results.

These are α Aquilæ and ζ Cygni. The declinations of α Aquilæ have been further corrected for a parallax of ″·20. For the other stars parallax has been ignored.

α Aquilæ.

Mean Date.	No. of Obs.	Weight.	Observed.	Computed.	O–C.
1857·4	219	9	$x-2\cdot1y=-$″·11	−″·09	−″·02
1873·2	120	5	$x-1\cdot0y=-\cdot04$	−·08	+·04
1890·8	138	6	$x+\cdot2y=\cdot00$	−·07	+·07
1902·4	196	8	$x+\cdot9y=-\cdot16$	−·07	−·09
1917·0	97	4	$x+1\cdot9y=\cdot00$	−·06	+·06

$x = -$ ″·075 = correction to Boss declination at mean epoch 1888·5.

$y = +$ ″·0081, $\frac{y}{15} = +$ ″·0005 = correction to Boss proper motion in declination.

The correction to Boss declination reduced to 1925·0 is

$$-\text{″}\cdot06 + \text{″}\cdot0005\ (T - 1925\cdot0).$$

ζ Cygni.

Mean Date.	No. of Obs.	Weight.	Observed.	Computed.	O–C.
1856·6	148	4	$x-2\cdot1y=-$″·18	−″·20	+″·02
1873·9	68	2	$x-1\cdot0y=-\cdot05$	−·02	−·03
1890·4	74	2	$x+\cdot1y=+\cdot12$	+·16	−·04
1903·0	137	4	$x+1\cdot0y=+\cdot32$	+·31	+·01
1915·9	117	3	$x+1\cdot8y=+\cdot44$	+·43	+·01

$x = +''\cdot143$ = correction to Boss declination at mean epoch 1888·5.

$y = +''\cdot1620$, $\frac{y}{15} = +''\cdot0108$ = correction to Boss proper motion in declination.

The correction to Boss declination reduced to 1925·0 is

$$+''\cdot54 + ''\cdot0108\,(T - 1925\cdot0).$$

These corrections are directly comparable with those derived by Eichelberger:

	α Aquilæ.	ζ Cygni.
Greenwich 1851–1925	$-''\cdot06+''\cdot001\,(T-1925\cdot0)$	$+''\cdot54+''\cdot011\,(T-1925\cdot0)$
Eichelberger	$+\cdot63+\cdot011\,(T-1925\cdot0)$	$+\cdot51+\cdot009\,(T-1925\cdot0)$

The first disagrees widely from Eichelberger, while the second agrees well.

When solutions were made giving equal weight (unity) to each fifteen-year group, the results were identical with those above.

4. It is as well, perhaps, at this point to give the collected results in brief, and in a comparative form, serving merely to indicate their general trend, and at a glance support or otherwise can be noted with Eichelberger's determinations.

The following, then, are the corrections to Boss's declination and annual proper motion for epoch 1925·0 derived from Greenwich observations 1851–1925, together with the Washington results for the fifty-five stars :—

Star.	Approx. 1925·0. R.A.	Approx. 1925·0. Dec.	Total No. of Obs.	Corr. to Boss Dec. 1925·0. Gr.	Corr. to Boss Dec. 1925·0. Eichelberger.	Corr. to Boss Annual P.M. Gr.	Corr. to Boss Annual P.M. Eichelberger.
	h m	° ′		″	″	″	″
α Andromedæ	0 4	+28 41	585	+·25	+·22	+·003	+·004
γ Pegasi	0 9	+14 46	368	+·38	+·64	+·011	+·011
β Andromedæ	1 6	+35 13	420	+·22	+·35	+·002	+·006
β Arietis	1 50	+20 27	367	+·13	+·42	+·007	+·007
α Arietis	2 3	+23 7	557	+·02	+·31	+·004	+·005
α Ceti	2 58	+ 3 48	281	+·26	+·53	+·005	+·009
η Tauri	3 43	+23 52	297	+·46	+·40	+·013	+·007
Aldebaran	4 32	+16 22	758	+·49	+·35	+·014	+·006
ι Aurigæ	4 52	+33 3	304	+·65	+·78	+·013	+·014
Rigel	5 11	− 8 17	499	+·44	+·28	+·007	+·005
β Tauri	5 22	+28 33	623	+·49	+·16	+·011	+·003
δ Orionis	5 28	− 0 21	264	+·21	+·51	−·002	+·009
ε Orionis	5 32	− 1 15	268	+·31	+·38	+·005	+·006
α Orionis	5 51	+ 7 24	660	+·19	+·33	+·001	+·006
μ Geminorum	6 18	+22 33	335	+·28	+·20	+·007	+·003
γ Geminorum	6 33	+16 28	376	+·37	+·29	+·010	+·004

Star.	Approx. 1925·0. R.A.	Approx. 1925·0. Dec.	Total No. of Obs.	Corr. to Boss Dec. 1925·0. Gr.	Corr. to Boss Dec. 1925·0. Eichelberger.	Corr. to Boss Annual P.M. Gr.	Corr. to Boss Annual P.M. Eichelberger
	h m	° ′		″	″	″	″
Castor	7 30	+32 3	572	+·13	+·61	+·004	+·007
Pollux	7 41	+28 13	946	+·20	+·30	+·004	+·005
α Hydræ	9 24	− 8 20	333	−·04	+·17	−·002	+·003
ε Leonis	9 42	+24 7	405	+·53	+·65	+·013	+·011
Regulus	10 4	+12 20	813	+·54	+·59	+·009	+·010
γ^1 Leonis	10 16	+20 13	438	−·07	−·02	+·002	−·001
δ Leonis	11 10	+20 56	463	+·60	+·64	+·014	+·011
β Leonis	11 45	+14 59	486	+·40	+·33	+·012	+·006
β Virginis	11 47	+ 2 11	246	+·35	+·68	+·008	+·011
ε Virginis	12 58	+11 22	364	+·18	+·52	+·001	+·009
Spica	13 21	−10 46	926	+·40	+·37	+·003	+·006
ζ Virginis	13 31	− 0 13	350	+·39	+·41	+·004	+·006
η Bootis	13 51	+18 46	603	+·37	+·35	+·009	+·006
Arcturus	14 12	+19 34	1344	+·40	+·55	+·009	+·009
ε^2 Bootis	14 42	+27 23	671	+·54	+·69	+·008	+·012
β Libræ	15 13	− 9 6	294	+·72	+·67	+·008	+·012
α Coronæ	15 32	+26 58	885	+·59	+·57	+·012	+·010
α Serp.	15 41	+ 6 40	535	+·39	+·55	+·008	+·010
δ Oph.	16 10	− 3 30	349	+·49	+·64	+·005	+·011
γ Herculis	16 19	+19 20	311	+·22	+·32	+·005	+·005
α Herculis	17 11	+14 28	391	+·65	+·70	+·014	+·012
α Oph.	17 31	+12 37	826	+·32	+·56	+·006	+·010
β Oph.	17 40	+ 4 36	540	+·35	+·70	+·007	+·012
μ Herculis	17 44	+27 46	530	+·61	+·68	+·012	+·012
72 Oph.	18 4	+ 9 33	284	+·01	+·36	+·002	+·005
η Serp.	18 17	− 2 55	385	+·09	+·52	·000	+·009
α Lyræ	18 34	+38 43	1158	+·07	+·30	+·004	+·004
β Lyræ	18 47	+33 16	529	+·16	+·35	−·002	+·006
ζ Aquilæ	19 2	+13 45	581	+·42	+·66	+·010	+·011
δ Aquilæ	19 22	+ 2 58	354	+·29	+·57	+·008	+·010
γ Aquilæ	19 43	+10 26	509	+·20	+·55	+·006	+·010
α Aquilæ	19 47	+ 8 40	770	−·06	+·63	+·001	+·011
β Aquilæ	19 52	+ 6 13	335	+·28	+·46	+·007	+·008
β Capric.	20 17	−15 1	211	+·24	+·37	+·005	+·006
ζ Cygni	21 10	+29 55	544	+·54	+·51	+·011	+·009
ε Pegasi	21 41	+ 9 32	362	+·17	+·61	+·005	+·011
α Aquarii	22 2	− 0 41	298	+·22	+·41	+·001	+·007
ι Pegasi	22 4	+24 59	252	+·87	+·73	+·019	+·014
α Pegasi	23 1	+14 48	346	+·37	+·56	+·009	+·010

The mean corrections to Boss are

Greenwich $+ ''\cdot33 + ''\cdot0068\ (T - 1925\cdot0)$
Eichelberger $+ ''\cdot47 + ''\cdot0080\ (T - 1925\cdot0)$

for a mean declination approximating roughly to $+ 10°$.

These stars, in the main, lie in the region $+ 30°$ to $- 10°$, and indicate also corrections to Boss proper motions varying with declination:

	$+30°$ to $+20°$.	$+20°$ to $+10°$.	$+10°$ to $0°$.	$0°$ to $-10°$.
Corrections to Boss P.M.	$+''\cdot009$	$+''\cdot009$	$+''\cdot005$	$+''\cdot003$
Number of stars	15	14	10	9

It is interesting here to note that twenty-eight stars in the above list are common to those (mentioned before) of Mr. Thackeray in 1896, and give the following deduced mean correction to the Boss P.G.C. proper motions:—

Mr. Thackeray in 1896	$+ ''\cdot0054$.
Greenwich 1851–1925	$+ \cdot0064$.

5. In an investigation of this character, the probable errors attaching to the results are not easy to assign.

The mean residuals (O–C) for each fifteen-year group given by the weighted solutions are:

Residuals (O–C).

	1851–1865.	1866–1880.	1881–1895.	1896–1910.	1911–1925.
Mean	$-''\cdot02$	$-''\cdot02$	$+''\cdot06$	$+''\cdot04$	$-''\cdot07$

where these quantities have a probable error only slightly larger than $\pm ''\cdot01$.

The mean values in each group give an indication of the smallness of the systematic error existing in the individual groups.

It may be of interest to note that if a systematic correction, say, of $''\cdot15$ were applied to a group at either end it would affect the deduced annual proper motions by $''\cdot002$.

Further, to obtain some idea as to the effect of the weighting, an alternate solution was made for every star, giving unit weight to each group.

The two solutions gave:

	Corr. to Boss Dec. at 1888·5.	Corr. to Boss Annual P.M. in Dec.
Weighted result	$+''\cdot088$	$+''\cdot0068$
Unit weight to each group	$+\cdot083$	$+\cdot0066$
Difference	$+\cdot005$	$+\cdot0002$
Average mean discordance	$\pm\cdot011$	$\pm\cdot0006$

One other point concerns latitude variation. Corrections similar to those affecting the observations in the last two groups have been

applied to the earlier ones. This mean correction depends on the annual term, and enters into the positions as a slight periodic effect depending on R.A.

Finally, the evidence deduced from the investigation suggests that this method of dealing with the Greenwich declination observations, although not entirely free from assumptions, probably gives better results than can be obtained by direct use of the published catalogues.

It is proposed to make a similar discussion of a larger number of stars.

Royal Observatory, Greenwich:
1927 May 3.

Theory of the Outer Layers of a Pulsating Star.

By A. S. Eddington, F.R.S.

1. The mathematical theory of a star pulsating symmetrically was investigated in two papers published in 1918–19.* Throughout the main part of the star the oscillations are very nearly adiabatic, and the effect of the periodic flow of heat is negligible just as in the ordinary theory of sound waves. But in the outer layers, beginning at some depth well below the photosphere, the adiabatic approximation ceases to be valid. The problem of obtaining a mathematical theory for these layers was left unsolved; and the scope of the investigation was limited to those phenomena which from their nature could not be appreciably influenced by the behaviour of the outermost shell of the star.

The outstanding phenomenon which demands an investigation of the non-adiabatic region is the observed phase relation of the light-curve and velocity-curve of a Cepheid variable. The maximum outflow of heat from the star's surface occurs at the time when the star is expanding most rapidly. According to theory the maximum outward flow in the interior is a quarter-period earlier, viz. at the time of greatest compression. It would seem, therefore, that between the central adiabatic region and the photosphere there must be a region of transition where the phase of the outflowing heat is gradually retarded through a quarter-period. We have to examine whether this lag can be accounted for theoretically.

The only result that can be foreseen without a definite mathematical theory of the transition region is that, as soon as the adiabatic approximation fails, the phase begins to lag. This naturally encouraged the hope that the lag would mount up to the required value, and that a mathematical investigation would reveal why it tended to reach a quarter-period at the boundary of the star.†

After many attempts to investigate the outer region, which proved fruitless, I have reached the theory contained in this paper which seems to contain some substantial progress, although the result is disappointing.

* *Monthly Notices*, **79**, 2, 177. † *Loc. cit.*, p. 181.

It shows that (so far as the factors at present recognised in the problem are concerned) the lag of phase will not grow to any important extent, and therefore the quarter-period retardation is not accounted for.

The bearing of this negative result on theories of Cepheid variation is discussed in § 10. It appears that the difficulty is probably common to all theories that are at all likely to find favour, and is not a special handicap of the pulsation theory.

2. *Notation and Numerical Values.**—Let p, ρ, T, g be the pressure, density, temperature, and acceleration of gravity at a point distant ξ from the centre. Let F be the net outflow of radiant heat across a sphere of radius ξ, and H the flux per sq. cm., so that

$$\mathrm{F} = 4\pi\xi^2\mathrm{H} \quad . \quad . \quad . \quad . \quad (2.1)$$

We fix attention on a particular element of matter and denote the equilibrium value of ξ by ξ_0; we write

$$\xi = \xi_0 + \xi_0\xi_1 \quad . \quad . \quad . \quad . \quad (2.2)$$

with similar definitions for all the other variables. Quantities with suffix 1 accordingly contain a time-factor e^{int}; their squares and products are neglected.

The relation between p_1 and T_1 in *adiabatic* oscillations is taken to be

$$\mathrm{T}_1 - \theta p_1 = 0 \quad . \quad . \quad . \quad . \quad (2.3)$$

and the coefficient of opacity is taken to vary as p/T^λ, so that

$$k_1 = p_1 - \lambda\mathrm{T}_1 \quad . \quad . \quad . \quad . \quad (2.4)$$

According to the current theory of opacity, λ is expected to be about $4\frac{1}{2}$; θ (which in the usual notation is $(\gamma - 1)/\gamma$) must be between $\frac{1}{4}$ and $\frac{2}{5}$.

The following symbols will also be introduced :—

$$\left.\begin{aligned} \sigma &= n^2\xi_0/g_0 \\ \alpha &= p_0/g_0\rho_0\xi_0 \\ \mathrm{P} &= 4\theta\mathrm{H}_0/n\xi_0 \\ x^2 &= 4\alpha p_0/\mathrm{P} \end{aligned}\right\} \quad . \quad . \quad . \quad . \quad (2.5)$$

σ and α are pure numbers and P has the dimensions of a pressure.

Near the surface of δ Cephei we have

$$\mathrm{H}_0 = 5{\cdot}10^{10} \quad \xi_0 = 2{\cdot}10^{12} \quad n = 1{\cdot}3.10^{-5} \quad g_0 = 300 \quad \sigma = 1{\cdot}2.$$

In the Cepheids generally σ is a little over unity.† Taking $\theta = \frac{1}{4}$, we have

$$\mathrm{P} = 2000.$$

The quantity α is the reciprocal of the quantity μ in Table III. (*M.N.*,

* Up to a certain point use is made of the mathematical development given in my earlier papers and in *Internal Constitution of the Stars*, §§ 127, 131, 132. This part is restated here in a very condensed form.

† *Internal Constitution of the Stars*, equation (131·3).

79, 10). It will appear later that its value is about 0·01 in the region with which we shall be chiefly concerned.

3. *Storage of Heat.*—Consider a column of material extending from the surface to a depth where the pressure is p. Let there be a phase retardation ϵ in this distance, so that the periodic flux at the surface being H_0H_1, the flux at p is $H_0H_1e^{i\epsilon}$. The difference

$$H_0(e^{i\epsilon} - 1)H_1$$

must be stored in the column.

Taking the pressure equation in the usual form

$$p = \mathfrak{R}\rho T/\mu \quad . \quad . \quad . \quad . \quad (3.1)$$

the specific heat at constant pressure is $\mathfrak{R}/\theta\mu$. Hence, considering only the temperature changes due to the storage, we have

$$\begin{aligned} H_0(e^{i\epsilon} - 1)H_1 &= \frac{\mathfrak{R}}{\theta\mu}\int\frac{dT}{dt}\rho_0 d\xi_0 \\ &= \frac{\mathfrak{R}in}{\theta\mu}\int\rho_0 T_0 T_1 d\xi_0 \\ &= \frac{\mathfrak{R}in}{\theta\mu}\int\frac{T_0T_1}{g_0}dp_0 \qquad (\text{since } dp_0 = -g_0\rho_0 d\xi_0) \\ &< \frac{\mathfrak{R}in}{\theta\mu}\frac{T_0T_1}{g_0}p_0 \end{aligned}$$

where the maximum values of T_0, g_0 and T_1 are to be used. Hence by (3.1)

$$\begin{aligned} e^{i\epsilon} - 1 &< \frac{inp_0^2}{\theta g_0\rho_0 H_0}\frac{T_1}{H_1} \\ &< ix^2T_1/H_1 \quad . \quad . \quad . \quad . \quad (3.2) \end{aligned}$$

by (2.5). It is scarcely possible for T_1 to exceed H_1; in any case T_1 cannot exceed unity, and H_1 from the observed variation of Cepheids is about 0·3. When ϵ is small (3.2) reduces to

$$\epsilon < x^2T_1/H_1 \quad . \quad . \quad . \quad . \quad (3.3)$$

so that no important change of phase can occur between the surface and a depth where x^2 is small compared with unity. The physical reason is that the small quantity of material in such a region is insufficient to store the heat that would accumulate if there were any appreciable delay between reception and emission.

We have to distinguish three regions of the star which we shall call A, B, C. In the central (adiabatic) region A the phase of H_1 is constant because the temperature variation which determines the flow is caused entirely by the compression and expansion. In the outermost region C the phase is also constant but for a different reason, as stated above. Our interest centres round the transition region B where change of phase is not forbidden by either condition.

To understand how diametrically opposite conditions in A and C

can give the same steadiness of phase, we must remember that the heat flow H_1 is both a cause and an effect of the temperature distribution involved in T_1, so that there is a linkage of H_1 to itself expressed by a differential equation. When heat-capacity is very great or very small so that an infinite or zero factor is introduced, the equation degenerates and the conditions are simplified. But there must evidently be an intermediate region where the linkage is effective, and the equations determining the phase are more complex.

It is necessary to form a general idea of the situation of region B in order to know what numerical values and approximations are appropriate. By (3.3) the point where $x^2 = 1$ is in the midst of the region, and we may take $x^2 = 0{\cdot}1$ and 10 as conventional boundaries. It will appear in due course from the solution for the region that these limits are wide enough. The following values near the two boundaries have been calculated for δ Cephei from the ordinary theory of the equilibrium state by the aid of Emden's tables :—

x^2.	ξ_e.	p_0.	T_0.	$1/a$.
0·10	6·75	7,100	39,000°	182
13·0	6·50	450,000	110,000°	70

The distance ξ_e from the centre is here given in Emden's unit, $\xi_e = 6{\cdot}9$ being the boundary of the star.

In the photosphere p_0 is of order 10 to 100, so that we are here concerned with a much deeper level. Our inner boundary is at a depth equal to $\frac{1}{17}$ of the star's radius. Less than 1/200,000 of the mass of the star is above this boundary, so that the adiabatic pulsation of the main mass suffers no interference from this more complicated region.*

4. *Formulation of the Ideal Problem.*—The observed phase relation of light-curve and velocity-curve occurs regularly in Cepheids comprising a wide range of mass and density. We might therefore expect it to be a rather general property of pulsating matter in anything approaching stellar conditions, and allow ourselves all reasonable latitude in idealising the problem. In any case it is desirable to avoid introducing complications which are apparently inessential, not only for the sake of the mathematical analysis but because, if it should turn out that the phase-relation must be ascribed to a particular stellar property to which no importance has hitherto been attached, that is just the kind of result we are anxious to find out. We need the solution of an ideal problem as a peg on which to hang discussion ; the effects of the various possible complications can then be examined one by one.

We are dealing with a comparatively thin shell or " blanket " which plays no mechanical part in the pulsation but is constrained to move out and in following the expansion and contraction of region A. We may therefore take ξ_1 to be prescribed and adopt the same value † for it throughout the thin shell.

* The mechanical interference is negligible because of the small mass ; the thermodynamical interference is negligible because that is the condition defining an adiabatic region.

† The consequences of a varying ξ_1 are examined in § 8.

The value of p_1 is also approximately constant and prescribed in the shell, where it is approaching the limiting value (*loc. cit.*, p. 13, equation (17*a*))

$$p_1 = -(4+\sigma)\xi_1 \quad . \quad . \quad . \quad . \quad (4.1)$$

The fact that ξ_1 and p_1 are sufficiently determinate in the region simplifies the problem to an extent which makes progress possible. (It was a failure to realise this earlier which has so long delayed the present investigation.) In the exact equations the determination of ξ_1 and p_1 on the one side and T_1 on the other side are interdependent, and the elimination gives a differential equation of the fourth order. In region A, T_1 is eliminated by the approximate equation $T_1 = \theta p_1$ instead of by the exact differential relation, and the equation then reduces to the second order. In region B, p_1 and ξ_1 are replaced by prescribed quantities instead of being eliminated by a differential relation, and there remains an equation for T_1 of the second order.

The approximation $\xi_1, p_1 = \text{const.}$ must, of course, only be employed after all differentiations have been performed. (The differentiated form $\xi_1' = 0$, would prescribe ρ_1 and therefore T_1, so that it is not a valid approximation.)

5. *Differential Equation for Region B.*—We shall suppose that the source of stellar energy is interior to B, so that the outward flux of heat receives no increment in passing through B. This condition gives

$$F_0 = \text{const.} \quad . \quad . \quad . \quad . \quad (5.11)$$

The usual stellar model will be taken in which

$$p_0 \propto T_0^4 \quad . \quad . \quad . \quad . \quad . \quad (5.12)$$

We pay no special attention to radiation-pressure, which is perhaps less important in the outer part of the star than in the interior. In so far as it is important, it is supposed to be included in p. It may be remarked that the inclusion in p of the radiation-pressure $\frac{1}{3}aT^4$ is not likely to *help* to bring about a phase difference between p_1 and T_1.

From the fundamental equation of radiative equilibrium

$$H = -\frac{ac}{3k\rho}\frac{dT^4}{d\xi} = -\frac{ac\xi^2}{3k\rho_0\xi_0^2}\frac{dT^4}{d\xi_0}$$

we have by logarithmic differentiation

$$H_1 = 2\xi_1 - k_1 + \frac{4d(T_0^4T_1)}{d(T_0^4)} \quad . \quad . \quad . \quad (5.2)$$

Hence, using (2.1), (2.4), and (5.12),

$$F_1 = H_1 + 2\xi_1 = 4\xi_1 - p_1 + \lambda T_1 + \frac{4d(p_0T_1)}{dp_0} \quad . \quad (5.25)$$

Now (*loc. cit.*, p. 13, equation (17))

$$\frac{d(p_0p_1)}{dp_0} = -(4+\sigma)\xi_1 \quad . \quad . \quad . \quad (5.3)$$

Hence
$$F_1 = 4\xi_1 - p_1 + \lambda T_1 + \frac{4d\{p_0(T_1 - \theta p_1)\}}{dp_0} - 4\theta(4+\sigma)\xi_1$$
$$= 4(1 - 4\theta - \theta\sigma)\xi_1 - (1-\lambda\theta)p_1 + \lambda y + \frac{4d(p_0 y)}{dp_0} \quad . \quad (5.4)$$

where
$$y = T_1 - \theta p_1 \quad . \quad . \quad . \quad . \quad (5.5)$$

Hence, remembering that $dp_0 = -g_0\rho_0 d\xi_0$ and that σ is proportional to ξ_0^3,

$$\frac{dF_1}{d\xi_0} = 4(1 - 4\theta - \theta\sigma)\xi_1' - 12\theta\sigma\frac{\xi_1}{\xi_0} + (1-\lambda\theta)g_0\rho_0\frac{dp_1}{dp_0} - g_0\rho_0\left(\lambda\frac{dy}{dp_0} + \frac{4d^2(p_0 y)}{dp_0^2}\right) \quad (5.6)$$

If the pressure were constant we should have
$$-\frac{1}{4\pi\rho_0\xi_0^2}\frac{d(F_0F_1)}{d\xi_0} = c_p\frac{dT}{dt},$$

both sides being expressions for the gain of heat per unit mass. By (5.11) this reduces to
$$-\frac{H_0}{\rho_0}\frac{dF_1}{d\xi_0} = c_p\, in\, T_0T_1 \quad . \quad . \quad . \quad (5.65)$$

and since $c_p = \mathfrak{R}/\mu\theta$
$$-\frac{dF_1}{d\xi_0} = \frac{inp_0}{\theta H_0}T_1 = \frac{4i}{\xi_0}\frac{p_0}{P}T_1.$$

When p_1 is not zero a term in p_1 must be added, and it is evident that the coefficient must be such as to give the combination $T_1 - \theta p_1$ which vanishes when there is no gain of heat. Hence the general formula is
$$-\xi_0\frac{dF_1}{d\xi_0} = \frac{4ip_0 y}{P} \quad . \quad . \quad . \quad . \quad (5.7)$$

Inserting this in (5.6) and eliminating ξ_1' by the relation (*loc. cit.*, p. 9 (5)),
$$-3\xi_1 - \xi_0\xi_1' = \rho_1 = p_1 - T_1 = -y + (1-\theta)p_1 \quad . \quad (5.75)$$

we obtain
$$\frac{ip_0 y}{P} + (1 - 4\theta - \theta\sigma)y - \frac{p_0}{\alpha}\left(\tfrac{1}{4}\lambda\frac{dy}{dp_0} + \frac{d^2(p_0 y)}{dp_0^2}\right) - 3(1-4\theta)\xi_1$$
$$-(1-\theta)(1-4\theta-\theta\sigma)p_1 + \tfrac{1}{4}(1-\lambda\theta)\frac{p_0}{\alpha}\frac{dp_1}{dp_0} = 0,$$

or
$$\frac{d^2y}{dp_0^2} + (2+\tfrac{1}{4}\lambda)\frac{1}{p_0}\frac{dy}{dp_0} - \alpha y\left(\frac{1-4\theta-\theta\sigma}{p_0^2} + \frac{i}{p_0 P}\right)$$
$$+\{3(1-4\theta)\xi_1 + (1-\theta)(1-4\theta-\theta\sigma)p_1\}\frac{\alpha}{p_0^2}$$
$$+\tfrac{1}{4}(1-\lambda\theta)p_0\frac{dp_1}{dp_0} = 0 \quad . \quad . \quad . \quad . \quad (5.8)$$

We shall consider the case in which $\lambda\theta = 1$, so that the last term vanishes. This is equivalent to assuming that *adiabatic* compression does not alter the coefficient of opacity of stellar material. Actually θ must be rather greater than $\frac{1}{4}$ and λ is probably near $4\frac{1}{2}$, so that $\lambda\theta > 1$ and the opacity decreases a little with adiabatic compression. The possible effects of this particular maladjustment should, of course, be considered in their turn (§ 9); but since it is only one of several causes tending to increase the flow of heat at the time of greatest compression it does not seem essential to retain it in the first investigation.

As already explained, we can now replace ξ_1 and p_1 by constant approximate values. The value of σ is about 1·3; hence $p_1 = -5{\cdot}3\xi_1$. For brevity, we take the whole term in (5.8) to be equal to

$$+ 2a\xi_1/{p_0}^2,$$

the actual value of the coefficient being 1·3 for $\theta = \frac{1}{4}$ and 1·7 for $\theta = \frac{1}{3}$. The equation now reads

$$\frac{d^2y}{dp^2{}_0} + (2 + \tfrac{1}{4}\lambda)\frac{1}{p_0}\frac{dy}{dp_0} - \alpha y\left(\frac{1 - 4\theta - \theta\sigma}{{p_0}^2} + \frac{i}{p_0 \mathrm{P}}\right) + \frac{2a\xi_1}{{p_0}^2} = 0 \quad (5.9)$$

6. *Solution of the Equation.*—I am greatly indebted to Dr. P. Kapitza, who showed me that an equation nearly of the form here obtained could be transformed into Bessel's equation. Later, Professor E. T. Whittaker gave me assistance in generalising the transformation. Professor G. N. Watson suggested to me the use of Lommel functions for the refinement discussed in § 8.

We shall adopt P as the unit of pressure, so that $\mathrm{P} = 1$. Although α doubles its value (§ 3) in the extreme range that we consider, its change is small compared with that of p_0, and it probably will not make any essential difference if we adopt a constant mean value $(_1{}^1{}_0{}^1{}_0)$ throughout.

Let

$$\left.\begin{aligned} v^2 &= -4ia p_0 \qquad (0 < \arg v < \pi) \\ u &= {p_0}^{\frac{1}{2}+\frac{1}{8}\lambda} y \end{aligned}\right\} \quad . \quad . \quad (6.1)$$

Then (5.9) is transformed into

$$\frac{d^2u}{dv^2} + \frac{1}{v}\frac{du}{dv} + u\left(1 - \frac{(1 + \frac{1}{4}\lambda)^2 - 4a(1 - 4\theta - \theta\sigma)}{v^2}\right) = 2i\xi_1(-4ia)^{\frac{1}{2}-\frac{1}{8}\lambda} v^{\frac{1}{4}\lambda - 1}. \quad (6.2)$$

Since λ is probably in the neighbourhood of 4, we shall here take $\lambda = 4$. Also on account of the small value of α we neglect $4a(1 - 4\theta - \theta\sigma)$ in comparison with $(1 + \frac{1}{4}\lambda)^2$.* Hence (6.2) becomes

$$\frac{d^2u}{dv^2} + \frac{1}{v}\frac{du}{dv} + u\left(1 - \frac{4}{v^2}\right) = 2i\xi_1 . \quad . \quad . \quad (6.3)$$

A particular integral is

$$u = 2i\xi_1(1 + 4/v^2) \quad . \quad . \quad . \quad . \quad (6.4)$$

and the complementary function will consist of Bessel functions $J_2(v)$ and $Y_2(v)$. It can be shown from the asymptotic expansions

* The general solution without these simplifications is obtained in § 8.

(W. 199, (1) and (2)) * that, the imaginary part of v being positive, $Y_2 - iJ_2$ tends exponentially to zero and $Y_2 + iJ_2$ tends exponentially to infinity for large values of v. Hence only the former combination satisfies the conditions in a star, and

$$u = 2i\xi_1\{1 + 4/v^2 + A(Y_2(v) - iJ_2(v))\} \quad . \quad . \quad (6.5)$$

To determine A we must consider the boundary conditions towards the surface. In region C the storage of heat is negligible so that F_1 is constant. Since p_1 and ξ_1 are constant (5.25) gives

$$(1 + \tfrac{1}{4}\lambda)T_1 + p_0\frac{dT_1}{dp_0} = \text{const.}$$

Integrating we have (for $\lambda = 4$)

$$T_1 = a + b/p_0^2 \quad . \quad . \quad . \quad . \quad (6.6)$$

At our upper boundary of region B the term b/p_0^2 has something like 10^{-4} of its photospheric value, which, of course, cannot exceed unity; consequently T_1 has become constant. Moreover, since p_1 is constant, our boundary condition is that y is constant for smallish values of v.

The phrase "constant" in the foregoing argument need not be scrutinised too closely. It is sufficient that it excludes singularities in y ($= u/p_0$) of the form v^{-2}, v^{-4} which would be given by the two terms in (6.4); accordingly the complementary function must be so chosen as to wipe them out.

Using the expansions of Y_2 and J_2 for small values of v, we find that this condition gives $A = \pi$. Then (6.5) becomes

$$u = 2i\xi_1\{1 + 4/v^2 + \pi(Y_2(v) - iJ_2(v))\} \quad . \quad . \quad (6.7)$$

and the expansion is

$$u = 2i\xi_1(\tfrac{1}{4}v^2\{\log(-\tfrac{1}{2}iv) + \gamma - \tfrac{3}{4}\} + \text{terms in } v^4) \quad . \quad (6.8)$$

where γ is Euler's constant 0·577. Hence

$$y = 2a\xi_1\{\log(-\tfrac{1}{2}iv) + \gamma - \tfrac{3}{4}\} + \text{terms in } v^2 \quad . \quad (6.9)$$

and there is no singularity (other than a logarithmic singularity).†

So far as the phase relation of light-curve and velocity-curve is concerned, the result (6.9) contains the answer required. But we naturally wish to trace just what has happened in the transition region, and for this a numerical calculation from (6.7) is necessary.

7. *Numerical Calculation.*—We are chiefly interested in the results in a range including $|v| = 1$ where neither the ascending nor the descending series are convenient for calculation. In order to use the

* The references W. are to the pages and formulæ in Watson's *Theory of Bessel Functions.*

† In physical problems logarithmic singularities do not count, since the approximations must inevitably become invalid before the logarithm grows large. In the present problem we should have to take account of the variation of a. In fact if (for very small values of v) we were to accept (6.9) as it stands the decrease of a would prevent y from increasing.

published numerical tables it is necessary to express the result (6.7) in terms of *ker* and *kei* functions. We have

$$\mathrm{Y}_n(v) - i\mathrm{J}_n(v) = \frac{\mathrm{J}_n(v)\cos n\pi - \mathrm{J}_{-n}(v)}{\sin n\pi} - i\mathrm{J}_n(v) \quad \text{(W. 64(1))}$$

$$= e^{-\frac{1}{2}in\pi} \cdot \frac{\mathrm{J}_n(v)e^{-\frac{1}{2}in\pi} - \mathrm{J}_{-n}(v)e^{\frac{1}{2}in\pi}}{\sin n\pi}$$

$$= e^{-\frac{1}{2}in\pi} \cdot \frac{\mathrm{I}_n(v/i) - \mathrm{I}_{-n}(v/i)}{\sin n\pi} \quad . \quad \text{(W. 77)}$$

$$= -(2/\pi)e^{-\frac{1}{2}in\pi}\mathrm{K}_n(v/i) \quad . \quad . \quad \text{(W. 78(6))}$$

Hence

$$\tfrac{1}{2}\pi(\mathrm{Y}_2(v) - i\mathrm{J}_2(v)) = \mathrm{K}_2(v/i)$$

$$= \mathrm{K}_0(v/i) - \frac{2i}{v}\mathrm{K}_0'(v/i) \quad . \quad \text{(W. 79)}$$

or setting $v = x\sqrt{(-i)}$

$$= \mathrm{K}_0(x\sqrt{i}) + \frac{2i}{x}\frac{d}{dx}\mathrm{K}_0(x\sqrt{i})$$

$$= \ker x + i\,\mathrm{kei}\,x + \frac{2}{x}(i\,\mathrm{ker}'\,x - \mathrm{kei}'\,x) \quad . \quad \text{(W. 81(5))}$$

Hence

$$u = 2i\xi_1\{1 + \frac{4i}{x^2} + 2(\ker x - \frac{2}{x}\mathrm{kei}'\,x) + 2i(\mathrm{kei}\,x + \frac{2}{x}\mathrm{ker}'\,x)\} \quad (7.1)$$

Note that x is the quantity already defined in (2.5). Also

$$\mathrm{T}_1 - \theta p_1 = y = u/p_0 = 4\alpha u/x^2 \quad . \quad . \quad (7.2)$$

The following table, showing the course of the quantities u and y, has been calculated from (7.1) with the aid of the table in *Brit. Assoc. Rep.*, 1915, p. 36:—

x.	$u/2i\xi_1$.	$y/8ia\xi_1$.
0·2	+·00775+·02478i	+·1938+·6195i
0·4	·03011 ·07170	·1882 ·4481
1	·16393 ·23159	·1639 ·2316
2	·47705 ·38200	·1193 ·0955
3	·74322 ·37083	·0826 ·0412
4	·90373 ·28587	·0564 ·0179
6	·99782 ·12930	·0277 ·0036

The table shows that the quarter-period retardation in passing through region B is not given by the theory. The wave of $\mathrm{T}_1 - \theta p_1$, which represents the change of temperature additional to that occurring through adiabatic compression, starts ($x = 6$) in the right manner in the phase of $i\xi_1$, *i.e.* in the same phase as the observed radiation of the star. But as its amplitude increases the phase regresses towards that of $-\xi_1$, so that on entering region C (between $x = 0{\cdot}4$ and $0{\cdot}2$) the phase is not much different from that of the ordinary adiabatic oscillation due to compression.

Moreover, the amplitude does not increase very much, and $T_1 - \theta p_1$ always remains insignificant. Taking $\alpha = \frac{1}{75}$, $\sigma = 1{\cdot}2$, $\theta = \frac{1}{4}$, we have at $x = 0{\cdot}2$

$$T_1 - \tfrac{1}{4}p_1 = (0{\cdot}0127 - 0{\cdot}0040i)p_1.$$

The whole effect thus only alters the amplitude of T_1 by about $\frac{1}{20}$, and of this change less than $\frac{1}{3}$ is in the direction required.

I would draw attention to the very commonplace character of the solution found in the table; it shows the ordinary smooth progress which we expect in a physical problem when the conditions are not specially critical. The staidness is not apparent until the formulæ are translated into numbers, since individual Bessel functions behave rather wildly. It is a fundamental assumption in the approximate treatment of natural problems that a small deviation from the adopted conditions leads to proportionately small deviation in the results; but in some cases this may fail owing to precarious conditions of balance, and the analysis may give a singular solution (referring only to the exact constants used) having no application in nature. The analytical form of (6.7) might suggest that there are very peculiar conditions to be represented, and lead us to suspect a singularity; but the numerical tabulation is entirely reassuring.

8. *More General Solution.*—Equation (6.2) is soluble in its most general form

$$\frac{d^2u}{dv^2} + \frac{1}{v}\frac{du}{dv} + u\left(1 - \frac{\omega^2}{v^2}\right) = Cv^{\tau-1} \quad . \quad . \quad (8.1)$$

where $\tau = \frac{1}{4}\lambda$. The solution that satisfies our boundary conditions is found to be

$$\begin{aligned} u/C &= S_{\tau,\omega}(v) + 2^{\tau-1}\Gamma(\tfrac{1}{2}\tau - \tfrac{1}{2}\omega + \tfrac{1}{2})\Gamma(\tfrac{1}{2}\tau + \tfrac{1}{2}\omega + \tfrac{1}{2})\cos\tfrac{1}{2}(\tau-\omega)\pi(Y_\omega(v) - iJ_\omega(v)) \\ &= s_{\tau,\omega}(v) - 2^{\tau-1}\Gamma(\tfrac{1}{2}\tau - \tfrac{1}{2}\omega + \tfrac{1}{2})\Gamma(\tfrac{1}{2}\tau + \tfrac{1}{2}\omega + \tfrac{1}{2})e^{\frac{1}{2}(\tau-\omega+1)i\pi}J_\omega(v) \quad . \quad . \quad (8.2) \end{aligned}$$

where S and s are the standard Lommel functions (W. 347(2)). The two expressions are equivalent, but the first is useful for large values of v and the second for small values. If ω is fractional the first expression must be appropriately modified, but the second holds without restriction. (The method of finding these expressions was first to show that $S + A(Y_\omega - iJ_\omega)$ is the general solution which makes y (or $u/v^{\tau+1}$) finite for v infinite, and then, after replacing S in terms of s, to determine A so that y remains finite at $v = 0$.)

We set $\tau + 1 = \omega + \epsilon$. By (6.2) ϵ is a small quantity of order α. Then by (6·1)

$$y = (-4i\alpha)^{\frac{1}{2}(\omega+\epsilon)}u/v^{\omega+\epsilon} \quad . \quad . \quad . \quad (8.3)$$

For small values of v the leading term in the expansion is

$$u/C = \frac{v^{\omega+\epsilon}}{(\omega+\epsilon)^2 - \omega^2} - 2^{\omega+\epsilon-2}\frac{\Gamma(\frac{1}{2}\epsilon)\Gamma(\omega+\frac{1}{2}\epsilon)}{\Gamma(\omega+1)}e^{\frac{1}{2}i\epsilon\pi}(\tfrac{1}{2}v)^\omega$$

so that

$$y = (-4i\alpha)^{\frac{1}{2}(\omega+\epsilon)}C\left(\frac{1}{2\omega\epsilon+\epsilon^2} - \tfrac{1}{4}(-\tfrac{1}{2}iv)^{-\epsilon}\frac{\Gamma(\frac{1}{2}\epsilon)\Gamma(\omega+\frac{1}{2}\epsilon)}{\Gamma(\omega+1)}\right) \quad (8.4)$$

When ϵ is small the two terms in the bracket become large, but their difference remains finite as ϵ tends to zero.*

The solution given in § 6 refers to the values $\omega = 2$, $\epsilon = 0$. The result (8.4) is the generalisation of (6.9), and enables us to test the effect of any likely deviation from the chosen values on the wave emerging from region B. It will be seen that the solution is entirely regular, that is to say, small deviations do not give rise to disproportionately large effects.

The result (8.4) can also be written in the form

$$y \cdot \frac{(-4ia)^{\frac{1}{2}(\omega+\epsilon)}C}{2\omega\epsilon + \epsilon^2}\left(1 - (-\tfrac{1}{2}iv)^{-\epsilon}\frac{\Gamma(1+\frac{1}{2}\epsilon)}{\Gamma(1)}\frac{\Gamma(1+\omega+\frac{1}{2}\epsilon)}{\Gamma(1+\omega)}\right) \quad (8{\cdot}5)$$

which shows the regular behaviour more clearly.

Our main purpose in introducing ϵ is not merely to take account of the very small quantity explicitly neglected in § 6, but also to relax some of the earlier approximations. The terms arising from various sources grouped together as $2a\xi_1$, in (5.9) can now be allowed to vary to any reasonable extent between the top and bottom of region B, the variation being represented as proportional to v^ϵ. To a large extent the changes in the physical constants of the material between the temperatures 39,000° and 110,000° can be absorbed into the equations in this way.

9. *Discussion of the Approximations.*—The negative conclusion reached in this investigation depends on the smallness of the quantity α. We have

$$\alpha = \frac{p}{g\rho\xi} = \int_\xi^R g\rho d\xi \div g\rho\xi = \frac{(g\rho)_m}{g\rho}\frac{R-\xi}{\xi}$$

where $(g\rho)_m$ is the mean value between ξ and R, which is evidently considerably less than $g\rho$. Further, since we are dealing with a depth rather small in comparison with the radius of the star, $(R-\xi)/\xi$ is small. Although the illustrative values of α found in § 3 refer to a simple model and may be altered if we take account of change of molecular weight, radiation-pressure, etc., in the outer part of the star, there is no escape from the fact that they are of the right order of magnitude.

A simplification, which I was inclined to regard with suspicion, is the adoption of a model in which $p_0 \propto T_0^4$. To test this we shall try the effect of adopting $p_0 \propto T_0^3$ in (5.2). In this case

$$\frac{4d(T_0^4T_1)}{dT_0^4} = 4T_1 + \frac{dT_1}{d(\log T_0)} = 4T_1 + \frac{3dT_1}{d(\log p_0)} = T_1 + \frac{3d(p_0T_1)}{dp_0},$$

so that (5.4) is altered to

$$F_1 = 4(1-4\theta-\theta\sigma)\xi_1 - (1-(\lambda+1)\theta)p_1 + (\lambda+1)y + \frac{3d(p_0y)}{dp_0}$$

and the solution proceeds as before with the modified coefficients. There are no new terms introduced, and the type of solution is unaltered.

* There is the usual logarithmic singularity if v also is zero. *Cf.* (6.9).

Another possibility is that the value of $\lambda\theta$ is such as to require retention of the term $p_0 dp_1/dp_0$ which was dropped from (5.8). This claims attention because, unlike the terms in ξ_1 and p_1, it is not multiplied by the small factor α. But it is itself a small quantity of order α, for

$$p_0\frac{dp_1}{dp_0} = -\frac{p_0}{g_0\rho_0}\frac{dp_1}{d\xi_0} = -\alpha\xi_0\frac{dp_1}{d\xi_0}$$

and by (4.1) $\xi_0 dp_1/d\xi_0$ is of order $3\sigma\xi_1$ unless the variation of density is abnormal.

Many other points for scrutiny have suggested themselves, but in most cases it is at once apparent that they yield nothing of importance.

10. *Theories of Cepheid Variation.*—The results reached in §§ 6–8 are independent of the theory of the deep interior. The only reference to great depth is in connection with the exclusion of a term $Y_\omega + iJ_\omega$ from the complementary function. At considerable depth this increases proportionately to $x^{-\frac{1}{2}}e^{x/\sqrt{2}}$, so that we need only go to a depth $x = 10$, say, in order to make sure that it is inadmissible. Nothing that happens below that level can reinstate it.

For example, it seems to be established by Dr Jeans' researches on viscosity that the angular velocity of rotation increases towards the centre of the star, so that the main mass may be rotating perhaps ten times faster than the surface. This will not affect our problem. If we should wish to modify the equations so as to take account of the star's rotation, only the surface value of the rotation is required.

Thus the problem of reconciling the theoretical and observed phase of the radiation from Cepheids becomes to a large extent detached from any particular view as to the nature of these variables. When we turn a spectroscope on the Cepheid it shows us that the varying radiation comes from a surface which is periodically advancing and receding. We fix attention on a layer of gas behind that surface, and, provided that the thickness of the layer is a small fraction of the radius of the star, we may assume that the whole layer advances and recedes with the surface. We can take the layer thick enough to include regions B and C without infringing the condition that it is a small fraction of the star's radius.

To calculate the radiation from the oscillating surface we must assume a certain amount of knowledge of the physical properties of the layer, such as the constants λ, θ, as well as sufficient insight into the laws of radiation, etc., operating there. But so far as this knowledge is concerned there is no differentiation between the various theories of Cepheid variables. Only in one point have we to make a decision: in the foregoing analysis it was assumed that the layer expanded laterally in phase with the displacement (so as to cover a larger surface). If this did not happen (orbital hypothesis) the corresponding terms in ξ_1 would be dropped.

Is any further knowledge required pertaining to anything outside the layer? If we concede that there is no source or sink of energy inside the layer, the *steady* influx of energy F_0 is determined by the

observed efflux. As already explained, the *periodic* influx F_0F_1 does not affect the periodic efflux, the layer having been chosen deep enough. Thus we require no thermodynamical data outside the layer. The only other external influence is the gravitational field, and it is here that the hypotheses may differ. Gravitational field should be understood in the sense of the theory of relativity as including centrifugal force and the reversed acceleration of the layer (though the latter is in any case fixed by the observations). The periodic variation g_1 may be greater in some theories than in others ; but it seems evident that they have this in common—the variation g_1 must necessarily have the same phase as the observed displacement, *i.e.* the *wrong* phase to account for the observed radiation.

Too many cases arise for individual discussion here ; but I can see no way of modifying g_1 so as to cure the discrepancy with observation, and apparently g_1 is the only relevant factor that we are at liberty to modify.

The old-fashioned theory of a resisting medium temporarily escapes the general slaughter because it denies our postulate (5.11), which asserts that there is no source of heat in regions B and C. If it were to be revived, some of the main objections would be removed by applying it to the pulsation theory instead of the orbital theory, *i.e.* we should assume that the star expanded and contracted against a resisting medium. But the required density of the medium seems to be quite prohibitive—as it was on the orbital theory.

To summarise the position—we have given a theory of an oscillating mass of gas forming the outer layer of a star which predicts that the radiation emitted will be nearly in phase with the displacement ; whereas it is a generally accepted fact of observation that the radiation from such a mass is in phase with the velocity.* Since the details of my investigation correspond to a hypothesis of the nature of Cepheid variation which I have hitherto held to be the most plausible, the result seems particularly hostile to that hypothesis. But this first judgment is modified when we realise that the details are not responsible for the result, and the same difficulty shows itself if, ignoring Cepheid hypotheses, we start directly from the observations and consider the general problem of a layer of gas oscillating to and fro in a field of force of unspecified nature.

Therefore I must conclude that there is some important physical circumstance affecting the outside layers of a star which has not been appreciated in formulating the conditions of the problem in §§ 4, 5. Many factors could be suggested for consideration—the electric and magnetic fields of the star, viscous forces arising from combined effects of rotation and expansion, relativity corrections, radiation-pressure in so far as its behaviour differs from gas-pressure, etc. I cannot claim to have reviewed these exhaustively, but I have not been able to pick out anything likely to achieve the object. And in this state I must leave the problem.

* It is of course possible that the observations are wrongly interpreted, and the spectral displacement is not a Doppler effect.

Postscript.

11. The foregoing paper was nearly ready for press when I received the March *Monthly Notices* containing a paper by J. J. M. Reesinck on the same problem. Except that the stated conclusion of the two investigations is the same, the papers have not very much in common. This is because Dr Reesinck's investigation concerns region C and mine concerns region B.

The most essential difference between our results is that, whereas Reesinck concludes (§ 9) that a small deviation from adiabatic oscillation at great depth decreases exponentially towards the surface, I find that it increases, though not sufficiently, nor in the right phase, to explain the Cepheid phenomenon.

I think that a comparison of the two papers will show that Dr. Reesinck's theory, whilst making considerable progress in a subject which had been nearly stationary for eight years, cannot be accepted without substantial emendation. Accepting his approximate equations (26), (27), these in my notation become

$$T_1 = a + \frac{b}{T_0^4} \quad . \quad . \quad . \quad . \quad (11.1)$$

since x_0 is proportional to T_0^4 by his equation (11). (My corresponding equation is (6.6), which gives b/T_0^8, because I include the variation k_1.) He attempts to find the constants a and b from boundary conditions at great depth and at the surface respectively. The constant b, however, is irrelevant to the heat-flow, since by (5.2)

$$H_1 = 2\xi_1 + 4d(T_0^4 T_1)/d(T_0^4) = 2\xi_1 + 4a$$

where, following Reesinck, k_1 is neglected. The constant a is the essential one. In § 8 he determines it on the assumption that the adiabatic region is reached before (11.1) ceases to be valid. This begs the question; he finds no change of phase because virtually he regards the beginnings of a change of phase as a signal to stop the search. In § 9 he relents and pushes the investigation further, when according to his own account the solution becomes wild and the amplitude increases exponentially in a manner impossible in a star. He misses the necessary inference that either—(*a*) he has chosen a solution that does not satisfy the boundary condition or (*b*) his approximations are insufficient. In either case the value of a remains unknown and no conclusion can be drawn as to the phase of the emergent heat-flow.

He challenges my statement that in the outer layers "the temperature oscillations are determined by the heat received and radiated rather than by compression and rarefaction." I think I should make it plain that I adhere to the statement. In fact, if proof is needed, it follows immediately from Reesinck's equations. In (26) he finds that $p = T_s(ad)$. But here $T_s(ad)$ is admittedly the boundary value; that is to say, the value to be used is not the adiabatic temperature oscillation for the region considered but for a region deep in the star. This shows at once that the temperature oscillation of an element is not determined by its own compression and expansion but by the radiation which

attunes it to the oscillations in deeper regions. Similarly my conclusion that $p_1 - \theta T_1$ is approximately zero, does not mean that the oscillations in region C are adiabatic. It is unlikely that the value θ_C near the photosphere is the same as the value θ_B at a much deeper level. My result is $p_1 - \theta_B T_1 = 0$, approximately, whereas the adiabatic condition is $p_1 - \theta_C T_1 = 0$.

I have tried to expose the fallacies unsparingly. May I therefore add that, as one who has lain fog-bound in these regions for eight years, I can well understand how they passed unnoticed.

On the State of Motion in the Galactic System.

By Bertil Lindblad, Ph.D.

(*Communicated by the Secretaries.*)

1. The aim of the present paper is to strengthen some arguments relating to a theory of the general state of motion of our stellar system which has been developed in a previous series of papers.* We assume that the stellar system has a general motion of rotation around an axis perpendicular to the galactic plane. The phenomenon of the "asymmetrical drift" of stellar velocities of great size, studied by Boss, Adams and Joy, Strömberg, Oort, and others, interpreted as due to a general decrease of the speed of rotation with increasing velocity dispersion, fixes the axis of rotation in the direction of the galactic longitude 330°. The direction of the rotation is retrograde, being from left to right for an observer situated to the north of the galactic plane. The direction towards the axis of rotation points very nearly towards the centre of distribution of the system of globular clusters according to Shapley's investigations.

The existence of such a general motion of rotation has received very strong support in a recent investigation by Oort † on the rotation effects in radial velocities and proper motions of distant galactic objects.

2. The interpretation of the phenomenon of the "asymmetrical drift" just mentioned implies that the stellar system may be divided formally into an infinite number of "sub-systems" of varying mean speed of rotation. The minimum of internal velocity distribution corresponds to the greatest speed of rotation, and thus defines the most rapidly rotating sub-system.

The formation of this particular state of equilibrium may be understood, at least partly, in the following way. Let us assume that the main part of the primitive stellar system once formed a mass of stellar and nebulous matter in a rather heterogeneous state of rotation, but on the whole very much flattened towards the galactic plane. As a possible origin of such a flattened system of high angular momentum we may

* *Arkiv för Matematik, Astronomi och Fysik*, 19 A, Nos. 21, 27, 35; 19 B, No. 7, 1925 and 1926. *Nova Acta Reg. Soc. Scient. Upsaliensis*, vol. extraord., ed. 1927.

† *B.A.N.*, **3**, 120, 1927.

picture a close encounter between two systems leading to a definite fusion. If W_1, T_1, W_2, T_2 are the potential and internal kinetic energy for the two systems, $\bar{T}_1$ and $\bar{T}_2$ the kinetic energy of translation relative to the centre of mass, for infinite distance between the two systems, and, finally, W_0 and T_0 the potential and kinetic energy of the combined mass, when it has reached a steady state after the encounter, we have,* according to Jeans,

$$W_0 = W_1 + W_2 + 2(\bar{T}_1 + \bar{T}_2),$$
$$T_0 = T_1 + T_2 - (\bar{T}_1 + \bar{T}_2).$$

If we ignore the last terms of the right-hand members, it is clear that the respective potential and kinetic energies simply add together.

This process of combination is, of course, an indirect one. The decrease of potential energy due to the mutual approach of the two systems is compensated by an increase of internal potential energy at the cost of internal kinetic energy, and the loss in the latter sort of energy is supplied by the increase in kinetic energy of the two component systems following the original decrease of potential energy. The contribution to the internal kinetic energy from this source is likely to involve a high angular momentum in the plane of the encounter. Of course the fusion may also be partial in the way that part of the matter recedes again to infinity. Or the steady state may be "ordinarily" unstable, which probably means that the system will intermittently develop some sort of spiral structure† in which matter recedes to a finite distance and then returns. Such a cataclysmic motion will in this case continue, until the character of the steady state has been modified into a stable form.

Part of the primitive system may be assumed to concentrate into a central nucleus of nearly spherical form, while the rest arranges itself in a flattened figure of rotational symmetry representing a steady state under the "regular" or "hydrodynamic" forces. A great amount of matter in this part of the system is likely to move in circular orbits in the equatorial plane, and constitutes the most rapidly rotating sub-system, while other parts of stellar matter move in orbits of various inclinations towards this plane. A division into sub-systems of varying speed of rotation may therefore certainly be made in the early stages of the new system. It is likely that the physical development, the degeneration and regeneration, of individual stars may proceed under somewhat different conditions in different sub-systems, and that such circumstances are responsible for a great part of the physical differences which have in several cases been found to follow the division into "sub-systems."

There is, however, a certain process which must lead, though extremely slowly, to a successive ejection of matter from the very flattened sub-systems. We assume these sub-systems to be originally very rich in matter and to contain stars of various masses. Let us consider a certain point of the system and introduce a co-ordinate

* *Monthly Notices, R.A.S.*, **82**, 138, 1922.

† Cf. *Arkiv för Matematik . . .*, 19 A, No. 35, 20 A, No. 10; *Monthly Notices, R.A.S.*, March 1927.

system following the circular motion of rotation of a particle at this point with one of the axes steadily directed towards the centre of the system. The orbits in this co-ordinate system for relative velocities, which are small compared with the rotational velocity, are very strongly curved, and the dimensions of the orbits decrease with decreasing relative velocity (*cf.* § 5). We will assume that the dispersion of relative velocities at the point considered is small compared with the velocity of rotation. It follows, then, that a passage of two stars in approximately hyperbolic orbits can only be considered to take place for small distances between stars. The amplitude of variation in the force from a distant star during a revolution of the system will be small even in relation to the order of magnitude of the force. The forces from stars at great distances may therefore be considered to act on a star only by their combined action in a resultant of " regular " force. Though the region of effective " passages " around a star must be restricted in this way, we must assume that the distribution of the relative velocities will at each point tend to approach a Maxwellian distribution. The stars with smaller masses must then in the mean acquire higher velocities relative to the rotating frame, and the effective thickness of their distribution at right angles to the galactic plane must increase.

It may be shown at once that the stars acquiring higher relative velocities in this way will have a tendency to lag behind the stars in the galactic stratum in speed of rotation. We assume that a star belonging originally to the most rapidly rotating sub-system in the galactic plane is ejected from this plane by a small component of velocity normal to this plane, and we consider the projection of the orbit on the galactic plane. With increasing height the component of the attracting force towards the axis of rotation will decrease, the projection of the orbit must therefore widen, and the star accordingly recedes outwards. The area constant of the star's motion must under all circumstances fall below the area velocity of the circular orbits in the galactic plane for these greater distances from the axis. The mean angular velocity of rotation of the star will therefore be smaller than the angular velocity of the circular orbits for the same mean distance from the centre.

It is clear that if we assume an ejection of this kind to take place at all points of the " most rapidly rotating sub-system," members of sub-systems of lower speed of rotation will occur up to a somewhat larger radius in the galactic plane than the limiting radius for the first-mentioned sub-system. We need not assume, however, that the increase in effective radius with decreasing speed of rotation is of an order of magnitude comparable with the radius itself. This circumstance will be of importance in the following discussion.

3. In order to get a quantitative estimation of the relation between mean speed of rotation and internal velocity dispersion, we must consider more closely the conditions of the steady state under the action of the " regular " forces. We assume in accordance with a theorem of Jeans,* which has also been given independently by Charlier,† that

* *Monthly Notices, R.A.S.*, **76**, 70, 1915.
† *Lund Meddelanden*, I., No. 82, 1917.

the frequency function of the six-dimensional element of volume in the generalised space of position and velocity is of the form $F(I_1, I_2)$, where I_1 and I_2 are the energy integral and the integral of areas on the galactic plane—*i.e.* in Jeans' system of notations,

$$I_1 = \Pi^2 + \Theta^2 + Z^2 - 2V, \quad I_2 = \varpi\Theta \quad . \quad . \quad (1)$$

ϖ, θ, and z are cylindrical co-ordinates, ϖ denoting the distance from the axis of rotation, θ an angular co-ordinate in the galactic plane, and z the vertical distance from this plane. Π, Θ, Z are the corresponding linear velocity components.

We may let the element $\varpi d\varpi d\theta dz d\Pi d\Theta dZ$ be formally represented by an element $dI_1 dI_2$ in a diagram that has I_2 and I_1 as axes. We consider the regions of finite $F(I_1, I_2)$ in this diagram. It is clear that we must have $I_1 < 0$, as the velocity of a star must fall below the velocity of escape. It is further clear that we have

$$I_1 > \frac{1}{\varpi^2} I_2^2 - 2V,$$

which means that for a given ϖ and z the region of finite frequency must lie between the axes $I_1 = 0$ and the parabola

$$I_2^2 = \varpi^2(I_1 + 2V) \quad . \quad . \quad . \quad . \quad (2)$$

The vertex of this parabola lies on the I_1-axis at a distance $I_1 = -2V$ from the origin. The parameter of the parabola for a given V is a maximum for points in the galactic plane.

It is of special interest to consider the envelope of the parabolas in the galactic plane, as this curve defines the entire area of finite $F(I_1, I_2)$. It may easily be verified that the envelope corresponds to the relation

$$I_2^2 = -\varpi^3 \frac{\partial V}{\partial \varpi},$$

and we then get from the signification of the parabolas according to the relations (1) and (2)

$$\Pi = 0, \quad Z = 0, \quad \Theta^2 = -\varpi \frac{\partial V}{\partial \varpi} \quad . \quad . \quad (3)$$

which corresponds to circular orbits in the galactic plane around the centre of the system.

We consider in fig. 1 the shape of the envelope for two different types of force. The full-drawn lines represent the case when the field of force is the same as if the mass could be dissected into homogeneous very flattened spheroids of the same equatorial radius which has been taken as unit of length. In this case $I_1 = 0$ corresponds to a circular motion along the equatorial boundary, and the envelope is a pair of straight lines $I_1 = \pm I_2 \sqrt{3\pi GM} - 2V_0$, intersecting at the point $I_2 = 0$, $I_1 = -2V_0$, where $V_0 = \frac{3}{4}\pi GM$ is the function of force at the centre of the system. In the diagram we have put $GM = 1$.

The dotted curves represent the case when the same mass is concentrated in a central nucleus of spherical form, and are given by the

equation $I_1 I_2^2 = -(GM)^2$. With decreasing radius of this nucleus the point of intersection with the I_1-axis is shifted towards $I_1 = -\infty$.

Oort's results, referred to above, indicate that the stellar system has a very considerable central condensation, so that we may assume the envelope curve to be of a character intermediate between the two curves of fig. 1.

Let us now consider the general distribution of relative stellar velocities as observed in the neighbourhood of the sun. We ignore for the moment the Kapteyn star-streaming, which will be considered later, and assume that for small velocities relative to the centroid, the velocity

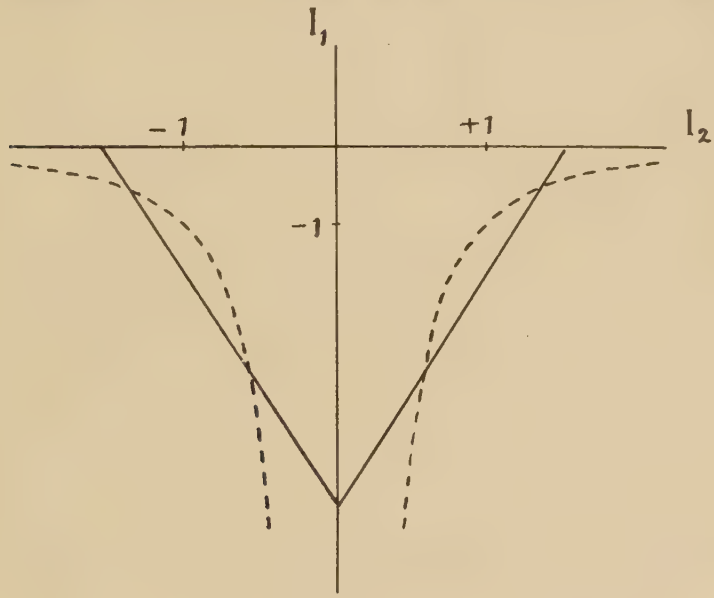


Fig. 1.

surfaces of equal frequency are nearly spherical. The velocity distribution as a whole for unit volume in our neighbourhood may then be pictured in the following way, which is shown schematically in fig. 2. We assume as an approximation to the observed distribution that in the velocity space the stars may be arranged in an infinite number of similar spherical distributions, which may be represented schematically by a series of velocity shells of decreasing radius with a uniform distribution on each shell. The radius of a shell is equal to the dispersion of the distribution. The area density on each shell increases at first rapidly with decreasing radius. The innermost shell is assumed to correspond to a circular motion around the centre.

In practice, however, the relation between velocity dispersion and mean motion has been found by determining the velocity dispersion and mean motion for groups of stars, defined either by the physical

constitution of the members or in a way more directly associated with the velocities. Strömberg, for instance, makes a division according to apparent magnitude and proper motion ($m + 5 \log \mu$), and then uses the radial velocities for determining the mean drift and the velocity dispersion. We assume here that the relation between these characteristics of a group is the same as if the group constituted exactly a spherical shell in our sequence.

As a consequence of an advance towards equipartition of energy in the velocities relative to the centroid, we must expect the inner velocity shells to contain on the average stars of higher mass and luminosity than the outer ones. This seems to be partly confirmed by

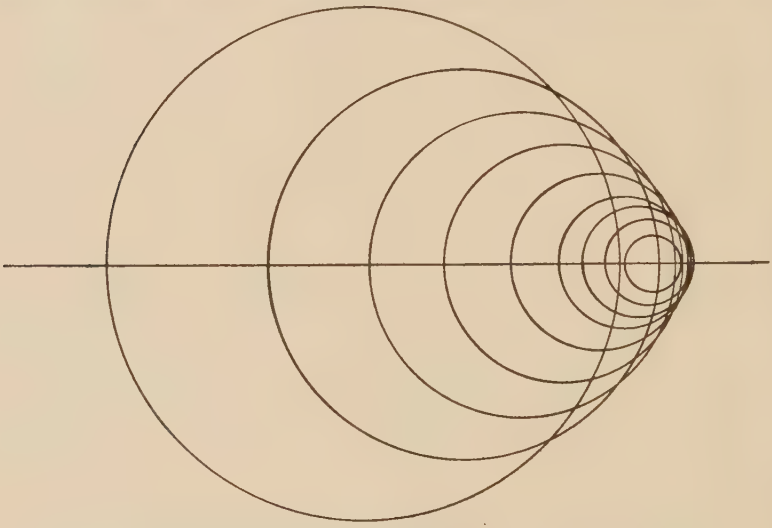


FIG. 2.—Schematic Dissection of the Observed Velocity Distribution into " Velocity Shells."

the observed data concerning the equipartition of energy in the translational motions relative to the centroid, but in some cases the equipartition process in question clearly cannot be of any significance—for instance, in relation to the high velocities within the sub-system constituted by the globular clusters. We must have to do in that case with a primeval cosmogonic cause the exact nature of which is, however, still completely obscure. On account of the general rotation of the system in the way discussed in § 2, the shells of greater radius have become shifted towards a lower mean speed of rotation. The character of the steady state under the " regular " forces does not, however, depend explicitly on the masses of the individuals.

A velocity shell has an immediate characteristic signification in the I_1, I_2-diagram of the system (fig. 3). It is represented simply by a uniform distribution along the part of a straight line

$$I_1 - kI_2 = c \quad . \quad . \quad . \quad . \quad (4)$$

which stretches across the characteristic parabola (2) valid for the point of the system considered. This may be verified immediately as follows. We have

$$\begin{aligned} I_1 - kI_2 &= \Pi^2 + \Theta^2 + Z^2 - 2V - k\varpi\Theta \\ &= \Pi^2 + (\Theta - \tfrac{1}{2}k\varpi)^2 + Z^2 - 2V - \tfrac{1}{4}k^2\varpi^2 = c. \end{aligned}$$

This corresponds to a spherical velocity shell with the mean velocity of rotation Θ_0 and with the radius R, where

$$\Theta_0 = \tfrac{1}{2}k\varpi, \quad R^2 = 2(V + \tfrac{1}{8}k^2\varpi^2) + c \quad . \quad . \quad (5)$$

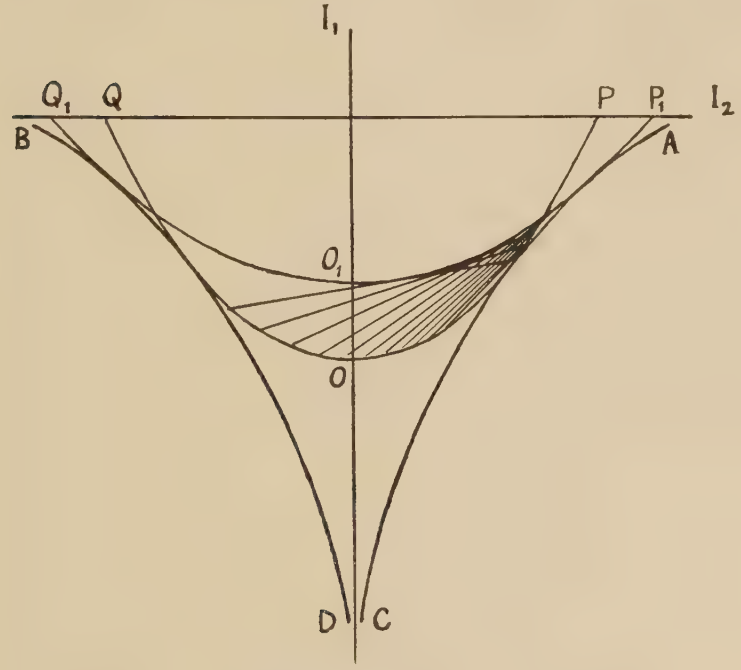


FIG. 3.—The Chords of the Parabola POQ correspond to the Velocity Shells of fig. 2.

which determines k and c in terms of Θ_0 and R. We remark that a point of the uniform distribution along one of the distribution lines in the I_1, I_2-diagram corresponds to all points in a narrow zone of a velocity shell, and thus does not correspond to an individual star.

The distribution of stellar velocities in our neighbourhood of space has thus been considered in the picture of a series of velocity shells which correspond to a series of chords of various inclinations in the characteristic parabola. As our point in the system lies nearly in the galactic plane, the characteristic parabola for this point will nearly touch the envelope curve. In fig. 3, AC and BD are the envelope curves and POQ is the characteristic parabola for our point of the system.

From the discussion in § 2 we derive the fundamental assumption that the different sub-systems, represented here by our series of velocity shells, have nearly the same maximum extension in the galactic plane. The curve $P_1O_1Q_1$ is the parabola corresponding to the limiting radius in this plane. The chords meeting $P_1O_1Q_1$ inside the parabola POQ must be tangents to $P_1O_1Q_1$, because otherwise the postulated uniform distribution on the chords within POQ would be interrupted. If our point of the system does not lie far from the limit, it is clear from the figure that we may assume as a general property of the chords that they are tangents to the limiting parabola $P_1O_1Q_1$. We now examine the consequences of this assumption for the relation between velocity dispersion and mean speed of rotation.

We introduce for convenience the notations

$$I_2 = x, \quad I_1 = y \quad . \quad . \quad . \quad . \quad (6)$$

and assume $\varpi = a$, $V = V_1$, at the limit of the system in the galactic plane. The equation for $P_1O_1Q_1$ then is

$$x^2 = a^2(y + 2V_1) \quad . \quad . \quad . \quad . \quad (7)$$

and for POQ

$$x^2 = \varpi^2(y + 2V) \quad . \quad . \quad . \quad . \quad (8)$$

The points of contact for the chords (4) on $P_1O_1Q_1$ may be denoted by x_1, y_1. The equation of one of these chords then is

$$xx_1 = \tfrac{1}{2}a^2(y + y_1 + 4V_1) \quad . \quad . \quad . \quad (9)$$

For the intersection between the straight line (9) and the parabola (8) we assume $x = x'$ and $x = x''$. From the significance of x in relation to the velocity shell,

$$x = \varpi\Theta,$$

and from the fact that the ends of the chord in POQ correspond to maximum and minimum of Θ in the velocity shell, we derive at once

$$\varpi\Theta_0 = \tfrac{1}{2}(x' + x''), \quad \varpi R = \tfrac{1}{2}(x' - x'') \quad . \quad . \quad (10)$$

Eliminating y and y_1 from equations (8), (9), and the relation

$$x_1^2 = a^2(y_1 + 2V_1),$$

we get for x' and x'' the expression

$$x = \frac{\varpi^2}{a^2}x_1 \pm \frac{\varpi}{a}\sqrt{2a^2(V - V_1) - x_1^2\left(1 - \frac{\varpi^2}{a^2}\right)} \quad . \quad (11)$$

If we insert in (10), and eliminate x_1, we find

$$\Theta_0^2 + \frac{\varpi^2}{a^2 - \varpi^2}R^2 = 2\frac{\varpi^2}{a^2 - \varpi^2}(V - V_1) \quad . \quad . \quad (12)$$

Introducing instead of R the velocity dispersion in a certain direction, *e.g.* the dispersion Z_0 at right angles to the galactic plane, we get

$$Z_0^2 = \tfrac{1}{3}R^2 \quad . \quad . \quad . \quad . \quad (13)$$

When ϖ is only slightly smaller than a, the right-hand member approaches to $-\varpi\frac{\partial V}{\partial \varpi}$, and thus we have

$$\Theta_0^2 + \frac{3\varpi^2}{a^2 - \varpi^2} Z_0^2 = -\varpi\frac{\partial V}{\partial \varpi} \quad . \quad . \quad . \quad (14)$$

We have Θ_0 = max. for $Z_0 = 0$. The "asymmetrical drift" is the quantity $(\Theta_0)_{max} - \Theta_0$.

The expression (14) may be compared with the corresponding relation derived in previous papers. The expressions only differ by the coefficient 3 instead of 2 in the second term of the left-hand member. The present relation, however, has been derived *without special assumptions concerning the shape of the equipotential surfaces* and the dependence of star density on the potential $\Omega_k = V + \frac{1}{8}k\varpi^2$. The old expression rested on the assumption that for a certain sub-system we have near the equatorial boundary a constant density within a spheroidal surface, which cannot be exactly true, because the space density ought to increase with Ω_k. In the present case the characteristic relation between Θ_0 and Z_0 has been found to follow from the sole conditions that the galactic radius of maximum extension for different sub-systems is approximately constant, and that our point of the system is fairly near the limiting radius.

We assume $(\Theta_0)_{max} = 300$ km./sec. as an approximate value from the drift velocity of the globular clusters, as computed by Lundmark * and by Strömberg.† The latter author's ‡ relation between asymmetrical drift and velocity dispersion is very well reproduced for

$$\frac{\varpi}{a} = 0{\cdot}84.$$

Our point of space should thus be situated 16 per cent. of the radius inside the galactic limit of the system. It would of course be premature to consider this result to be of more than a qualitative significance. It is evident, however, that the assumptions made have been reduced to a minimum, and that only observations of density and velocity distribution in fairly distant parts of the stellar system will make it possible to find starting-points for a refined theory.

It has to be remarked that we have not had to make any assumption about the frequency function $F(I_1, I_2)$ for the regions of the I_1, I_2-diagram below the characteristic parabola for our point of the system, *i.e.* for points of greater values of V, for instance, situated closer to the centre in the galactic plane. It is therefore not necessary to assume that the lines of uniform distribution continue as straight lines in those regions of the diagram.

When we proceed downwards along the right-hand envelope curve in fig. 3, we must expect to be forced to introduce new lines of distribution starting out in succession from the envelope curve in question.

* *Publ. Astron. Soc. of the Pacific*, **35**, 318, 1923.
† *Mount Wilson Contr.*, No. 292; *Astrophys. Journal*, **61**, 1925.
‡ *Ibid.*, No. 293.

In other words, we cannot expect all distribution lines to end as tangents of the parabola $P_1O_1Q_1$. The chords of a parabola POQ which correspond to the members of "the most rapidly rotating sub-system" for a point near the galactic plane must in the limit coincide with the common tangent of the envelope curve and the characteristic parabola, because they represent a circular motion in the galactic plane. According to the relation (3) this is identical with the condition that we shall always have for points in the galactic plane

$$(\Theta_0)^2_{\max} = -\varpi\frac{\partial V}{\partial \varpi} \quad . \quad . \quad . \quad . \quad (15)$$

exactly as in the relation (14), which is thus valid generally for a vanishing value of Z_0.

It is of course not necessary to identify formally "sub-systems" and "distribution lines." The former conception may be considered as wider, embracing several distribution lines, or even being composed of successive parts of distribution lines, as in the case of the "most rapidly rotating sub-system," which contains the infinitely small chords of distribution of successive characteristic parabolas.

4. Oort's results indicate that the gravitational field in our neighbourhood of the system may be approximately governed by a combination of two central forces, one part K_1 due to a central spherical mass, and one part K_2 proportional to ϖ and thus possibly due to a mass of approximately spheroidal figure, or to the sum of a series of spheroidal distributions with the same centre and equatorial plane. The ratio $\frac{K_1}{K_2}$ was estimated to be 0·29. If we assume that K_2 is caused by a series of very flattened homogeneous spheriods of total mass M, and the force K_1 by a mass μM at the centre, we obtain for the ratio of the two masses

$$\mu = 4{\cdot}8.$$

The degree of concentration of the system is thus very considerable, and the "effective flattening" in what concerns the distribution of force in the galactic plane is fairly low. The system has therefore in all probability "ordinary" stability with respect to mass motions,* and the spiral orbits, which on occasional disturbance might be ejected from the system as "mother system," will differ very little from the circular boundary in the galactic plane. In this way the conditions do not seem to allow a development of clear spiral structure in our stellar system.

5. Among the stars of low relative velocities near the galactic plane we know from observed conditions, and may also account theoretically for, a tendency to group together in more or less definite local formations, star-clouds or local systems. The gravitational field must therefore be expected to show local irregularities. A close passage between two clouds, one exterior and one interior, on account of a slight change in angular speed of rotation following the difference in distance from the centre of the system, must tend to convert the difference in angular

* Cf. *Arkiv för Matematik . . .*, 20 A, No. 10, 1927.

speed of rotation into a more irregular two-dimensional motion in the galactic plane. Once such streamings originate, they will pursue their orbits for long times under the general field of force of the system. We may inquire about the form of these orbits relative to a rotating frame under a force of the kind just considered above.

Let us assume that the co-ordinate system ξ, η follows the maximum speed of rotation $(\Theta_0)_{max}$ at the origin $\xi=0$, $\eta=0$, with the ξ-axis steadily directed towards the centre of the stellar system. For small motions in this co-ordinate system we obtain in the first approximation the elliptic orbits

$$\xi^2 + \left(\frac{\omega_1}{2\omega}\right)^2 \eta^2 = c^2 \quad . \quad . \quad . \quad . \quad (16)$$

where the origin has been chosen to coincide with the centre of the orbit. For the velocity components ξ', η' we get

$$\xi'^2 + \left(\frac{\omega_1}{2\omega}\right)^2 \eta'^2 = \omega_1^2 c^2 \quad . \quad . \quad . \quad (17)$$

In these formulæ ω is the angular speed of rotation of the most rapidly rotating sub-system at the point $\xi=0$, $\eta=0$. Putting

$$\beta = \frac{3\mu GM}{\varpi^3},$$

we have further

$$\left.\begin{aligned} \omega^2 &= \beta\left[\frac{\pi}{4\mu}\left(\frac{\varpi}{a}\right)^3 + \frac{1}{3}\right] \\ \omega_1^2 &= 4\omega^2 - \beta \end{aligned}\right\} \quad . \quad . \quad . \quad (18)$$

When μ is small and the spheroidal mass M predominates, we have $\omega_1 = 2\omega$. When μ is great and the mass at the centre determines the character of the field of force, we get $\omega_1 = \omega$. In the former case the orbits are circular, in the latter case ellipses with axes in the ratio 2 : 1, with the longer axis at right angles to the radius vector of the system. Let us assume, in rough accordance with Oort's results, that ω corresponds to a velocity $\Theta_0 = 300$ km./sec. for $\varpi = 5000$ parsecs. We then get in the case of a maximum velocity ξ' in the direction of the radius vector of 30 km./sec. as value of the smaller axis of the ellipse

$$c = 390 \text{ parsecs.}$$

An idea of the dimensions of the relative orbits for ordinary relative velocities has thus been obtained.

We consider now a multitude of such motions traversing in all directions a certain small region of the system. Though in the co-ordinate system ξ, η the velocity in an orbit is a maximum in tangential direction, if μ has a considerable value, the *net stream velocity* V_T measured relative to the local velocity of rotation $(\Theta_0)_{max}$ around the centre of the stellar system, and not as in the system ξ, η relative to a velocity of rotation Θ_0 corresponding to the value of ω for the centre of the particular relative orbit, is smaller in absolute amount than the maximum velocity $V_R = \xi'_{max}$ in the direction of the radius.

We get

$$\frac{V_T}{V_R} = \frac{2\omega c - \varpi(\Delta\omega)_c}{c\sqrt{4\omega^2 - \beta}} \quad . \quad . \quad . \quad (19)$$

where $(\Delta\omega)_c$ is the change of $\omega = \frac{1}{\varpi}(\Theta_0)_{max}$ on an interval of the radius equal to the smaller axis c of a relative orbit.

The ratio in question decreases from 1·0 to 0·5, when μ increases from 0 to ∞. We get by differentiating the relation

$$\omega^2 = GM\left(\frac{3}{4}\frac{\pi}{a^3} + \frac{\mu}{\varpi^3}\right),$$

$$\varpi(\Delta\omega)_c = \tfrac{3}{2}\omega c \frac{\mu}{\frac{3}{4}\pi\left(\frac{\varpi}{a}\right)^3 + \mu}.$$

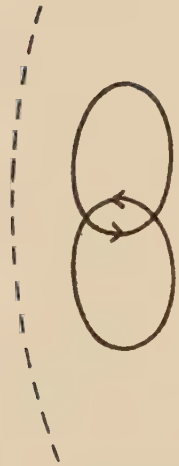


FIG. 4.—Relative Orbits in the Galactic Plane.

For $\mu = 4{\cdot}8$ and $\varpi = 0{\cdot}84a$ we then get

$$\frac{V_T}{V_R} = 0{\cdot}65.$$

The observed ratio of the axes of the velocity ellipsoid is thus approximately accounted for, if we assume that our point of space is traversed in all directions by streamings following similar orbits, carrying on the average an equal amount of matter per unit volume of the generalised space of position and velocity. Two orbits intersecting at opposite vertices have been pictured in fig. 4.

If we disregard the angle of about 20° which the mean observed vertex line forms with the direction towards the centre, the phenomenon of the Kapteyn star streaming seems to be in good accordance even quantitatively with the general state of motion of the galactic system as given by the present theory.

The Predicted Return of Comet Schaumasse (1911 VII, 1919 IV).

By G. Merton, M.A.

The orbit of this comet, from the observations of 1919, has been computed by G. Fayet and A. Schaumasse.* Although they had calculated the Jupiter perturbations from 1911 to 1919, no attempt was made to use them for linking up the two apparitions and improving the value of the mean motion. Their reason for this was that, in view of the approach (0·35 unit) of the comet to Jupiter in 1913, the mean motion employed in computing the perturbations 1911–1919 was considered not to be correct enough for the results to be of value.

As a matter of fact the period they used was in error by 18 days, so that the calculated approach to Jupiter was only about five days in error. It seems very unlikely, judging from similar cases, that the values of the perturbations so found are in error by more than a few per cent., if the calculations were otherwise correct. Even if the perturbations of the mean motion were in error by 10 per cent., the combination of the two apparitions would still lead to a prediction for 1927 uncertain from this cause by only about three days, whereas Schaumasse's computation based only on the 1919 observations predicts the return 49 days later.†

The purpose of this paper is to explain this large discrepancy, to show that the observational equations for 1919 will tolerate the large corrections to the elements required when the apparitions of 1911 and 1919 are combined, and to revise the prediction.

The investigation was based on the figures published by G. Fayet and A. Schaumasse in the *Journal des Observateurs* (vol. v., pp. 89–99). The values of the mean motion required by combining the two apparitions are given by

$$n = \frac{360^\circ - \Delta M}{N}.$$

Inserting the value of the total perturbation in the mean anomaly, $\Delta M = +3^\circ\ 38'\ 15''{\cdot}7$, and the interval $N = 2898{\cdot}44$ days, we have

$$n = 442''{\cdot}619 \quad \text{in 1911.}$$

Adding to this the perturbation $\Delta n = +4''{\cdot}192$, we get

$$n = 446''{\cdot}811 \quad \text{in 1919.}$$

The value obtained by Fayet and Schaumasse from the 1919 observations was $n = 439''{\cdot}476 \pm 0''{\cdot}7$. The probable error is, however, fictitious, as an examination of the least-squares solution will show. Although six-figure logarithms were used, the quantities were not known to this degree of accuracy and the final equation of the solution

$$0{\cdot}000016\,x = 0''{\cdot}00271$$

therefore means that the corrections and their probable errors so

* *J.O.*, v., No. 11.

† *Nice Obs. Bull.* 28. His results give T = 1927 Nov. 19·4 U.T.

deduced were not reliable. They expressed the corrections in terms of the mean anomaly, and after substituting in the original equations of condition obtained the best value of it.

For the present purpose the corrections to the elements were rearranged so as to express them in terms of the correction to the mean motion which is really at the root of the difficulty.* The original elements together with these corrections were then

M	7° 43′ 52″·15	− 22″·349 + 70·316 dn	
ω'	105 55 56·39	+ 328·971 − 78·292 dn	Equator 1919·0
Ω'	33 39 13·68	+ 9·210 − 3·716 dn	
i'	27 19 55·75	− 23·182 + 5·065 dn	
φ	45 14 32·38	− 69·876 − 114·459 dn	
n	438″·1470	+ 0·0 + 1·0 dn	

Epoch and Osculation: 1919 Dec. 23·0 G.M.T.

By substituting the corrections in the original equations of condition the O–C residuals were expressed in the form

	Residuals in α	Residuals in δ
I	− 0″·18 + ·19 dn	− 0″·33 + ·19 dn
II	− 0·45 − ·22 dn	− 1·80 + ·01 dn
III	+ 1·16 + ·13 dn	+ 2·78 − ·16 dn
IV	− 1·24 + ·21 dn	+ 0·92 − ·03 dn
V	+ 0·64 − ·21 dn	− 2·66 + ·18 dn

These gave for the sum of the squares

$$[pvv] = 22''\cdot46 - 2\cdot44\,\mathrm{d}n + 0\cdot283(\mathrm{d}n)^2,$$

for which the minimum is 17″·2 for dn = + 4″·34. This differs somewhat from their result dn = + 1″·33, and is nearer to the value + 8″·664 required after taking the 1911 apparition into account. That the observations will tolerate very large changes in the mean motion will be clear if against different values we tabulate the sums of the squares of the weighted residuals [pvv].

n	dn	[pvv]	[vv]	Perihelion, 1927
450″	+ 11″·853	34″·2	16″·9	Sept. 11·1
448	9·853	25·9	12·3	,, 24·0
446	7·853	20·8	9·2	Oct. 6·9
444	5·853	17·9	7·6	,, 20·0
442	3·853	17·3	7·5	Nov. 2·2
440	+ 1·853	18·9	9·0	,, 15·6
438	− 0·147	22·8	12·0	,, 29·0
436	− 2·147	29·0	16·6	Dec. 12·6

* Note a misprint in their paper, p. 97, line 5 from bottom, *read*:

$$u = [7\cdot554610_n]\,x + [0\cdot630897].$$

The sums of the squares of the unweighted residuals, [*vv*], have been included for comparison with the value 9″·8 for $n = 439''{\cdot}476$ deduced by Fayet and Schaumasse.

The figures in this table are unreliable for the bigger values of dn, since the other elements also would require such large corrections as to modify the values of the coefficients of the differential corrections on which the above results necessarily depend. It seems quite possible that the correction of $dn = +8''{\cdot}664$ would fit the observations better than here indicated, although even with the above figures the biggest error in any normal place would be less than $1\frac{1}{2}''$. It must also be remembered that there were only 14 observations available, covering an arc of 64 days, and that the first and last places rest on only one observation each.

We may conclude, therefore, that the 1919 observations are satisfied with the value

$$n = 446''{\cdot}811.$$

After inserting the corresponding value of $dn = +8''{\cdot}664$ in the other corrections, the following elements were obtained :—

Epoch and Osc. 1919 Dec. 23·0 G.M.T.

M	7° 53′ 39″·0		
ω	45 46 57·4	Ecliptic 1919·0	log a ·599922
Ω	90 37 31·0		log q ·067592
i	14 44 31·8		e ·706458
φ	44 56 50·8		Period 7·94114 years.
n	446″·811		

T 1919 Oct. 20·39588 G.M.T.

In the prediction for 1927 the Jupiter perturbations computed by F. R. Cripps for the *B.A.A. Handbook* prediction have been utilised as they go to 1927 November, while those by Schaumasse stop at July. The elements used in computing these were those published by Schaumasse for 1919, and it was not noticed then that no account had been taken of the previous apparition. The resultant perturbations, 1919–1927, will therefore be slightly in error, though this is of no account owing to the neglect of all other planetary attractions.

ΔM	− 15′ 34″·8	Δi	− 1′ 21″·6
$\Delta\omega$	+ 16 25·6	$\Delta\varphi$	− 1 12·9
$\Delta\Omega$	− 9 25·6	Δn	− 0·501

After applying these and making the necessary adjustments for precession, the resultant predicted elements were

T 1927 Oct. 1·54 U.T.

ω	46° 3′ 38″	1927·0	log a ·60025
Ω	90 34 33		log q ·06829
i	14 43 10		e ·70621
φ	44 55 38		Period 7·9501 years.
n	446″·31		

It may be of interest to note that had the perturbations been neglected throughout, the date predicted by using the observed interval of 2898 days between the perihelion dates of 1911 and 1919 would be but 4 days earlier than the above.

Unfortunately this revision makes the return an unfavourable one, but as the next one will be more so, it is very important that a thorough search should be made for the comet this time.*

The Royal Observatory, Greenwich:
1927 May 9.

Observations of Novæ, 1926–1927. By W. H. Steavenson.

Most of the following observations were made with the 6-inch Wray refractor. In the case of the fainter Novæ, however, advantage was taken of the kind permission of the Astronomer Royal to use the 28-inch equatorial of the Royal Observatory. Where an observation was made with this instrument it is marked with the letter G in the lists of estimates here given. The observation of Nova Persei, made on 1927 April 11, was made with the 18-inch refractor of the Radcliffe Observatory, Oxford, by kind permission of Dr. Knox-Shaw.

Nova Cassiopeiæ (1572).

The field of this Nova was examined on 4 nights between 1926 May 15 and August 22. No change was noted in the brightness of the small star suspected by some observers to be identical with Tycho's star; nor did any other star in the field exhibit any variability or otherwise appear in any way abnormal.

Nova Persei (1901).

Observed on 50 nights. Variability of an irregular character has again been manifested in the period covered by the observations; but the extreme range of brightness (almost exactly one magnitude) has been smaller than in some recent years, if we exclude the observation of May 31. No definite period can be deduced from the observations; but, as often noted before, several periodicities, long and short, appear to be mutually superimposed in the observed light-curve. The range of variability was considerably greater before the end of September than during the remainder of the period. The small wisp of nebulosity attached to the Nova was well seen with the 28-inch telescope on 1926 October 10. It does not appear to have changed appreciably in brightness or extent for many years.

* Provisional search ephemerides based on this revised date of perihelion have been published by Dr. A. C. D. Crommelin in the *B.A.A. Journal*, **37**, 240.

Date.	Mag.	Date.	Mag.
1926 May 31	12·53±	1926 Sept. 26	13·37 G.
July 12	13·18±	27	13·52
17	12·98	28	13·52
31	13·78 G.	30	13·72
Aug. 4	13·87	Oct. 1	13·72
11	13·77	9	13·67
14	13·62	10	13·73 G.
15	13·59	17	13·62 G.
18	13·47	31	13·72 G.
21	13·37	Nov. 2	13·83
24	12·84	7	13·8 ±
26	12·98	9	13·88
28	13·67	12	13·93
29	13·37	14	13·61
Sept. 4	13·88	28	13·78
7	13·03	Dec. 3	13·88
8	12·93	8	13·47
9	12·84	1927 Jan. 25	13·57
10	13·08	27	13·42
12	13·37 G.	29	13·67
13	13·28	30	13·52
17	13·08	Feb. 6	13·68 G.
18	12·93 G.	27	13·61 G.
19	12·98 G.	Mar. 27	13·18 G.
25	13·03 G.	Apr. 11	13·52 R.

Nova Geminorum (1912).

Observed on one night (1926 April 11) with the 28-inch telescope. It appeared to be one-tenth of a magnitude brighter than the small star of about 14 mag., two minutes of arc north preceding the nova. This star is the one referred to in *M.N.*, **86**, 368. The nebulous envelope in which the Nova is involved was again seen, but scarcely so well as on 1926 January 9, when the field was more favourably placed. The Nova is difficult to observe satisfactorily, owing to the presence of a bright companion, of about the 13th magnitude, a few seconds of arc to the north.

Wolf's Nova in N.G.C. 4303.

This object was observed on 3 nights during 1926 May. The first observation was made with the 28-inch refractor on 1926 May 16. The Nova was then estimated to be equal to or perhaps one-tenth of a magnitude fainter than the star "A," which precedes the nucleus of the nebula (see *M.N.*, **86**, 636). This star was estimated to be of

magnitude 13·5 ±. When next seen, with the same instrument, on May 30, the Nova was found to have faded considerably. It was considered to be $0^m.6$ fainter than A; that is, of magnitude 14·1 ±. On the following night, May 31, it was fairly glimpsed with the 6-inch Wray. A was a distinctly easier object, being quite steadily seen, and was estimated to be fully half a magnitude brighter than the Nova. The field was not observed again until 1927 May 1. On this occasion the Nova was quite invisible in the 28-inch telescope. The sky was transparent and A appeared extremely bright. The images of all stars, however, were poorly defined that night, so that the objective could not work to its limit as regards magnitude. It was, however, possible to decide that the Nova was certainly fainter than $15^m.0$, and probably fainter than $15^m.5$.

Nova (T) *Coronæ* (1866).

Observed on 24 nights. As in the previous year, some slight fluctuations of light were observed. The range is small ($0^m.2$), but as I believe the variations are at least partly real, I give the individual estimates below.

Date.	Mag.	Date.	Mag.
1926 Apr. 12	10·01	1926 July 7	9·91
13	10·01	9	9·91
15	10·06	12	9·91
16	10·01	13	9·91
17	10·01	14	9·96
19	10·06	15	9·91
May 3	10·06	20	9·91
15	10·06	23	9·91
31	10·11	30	9·96
June 10	10·01	Aug. 4	9·91
30	9·96	22	9·96
July 1	9·96	Sept. 10	10·11

Nova Ophiuchi (1848).

Observed on 12 nights. This star is certainly to be counted among the Novæ whose light is unsteady, but the range of its variability is much smaller than that of Nova Persei. The low altitude of the Nova makes it a somewhat difficult object to observe satisfactorily in a 6-inch telescope. The observations given below are too few and scattered to give by themselves much information as to the existence of a definite period in the variations. There is always something unsatisfactory about the appearance of this object as compared with that of other stars of the same brightness. As seen with the 28-inch telescope on 1926 July 11, the image of the Nova appeared to be not quite stellar, being closely involved in a nebulous haze, between one and two seconds

of arc in diameter. A neighbouring star of nearly the same brightness came to a slightly shorter focus, and was seen as a minute point of light.

Date.	Mag.	Date.	Mag.
1926 May 3	12·35	1926 July 9	12·10
15	12·70	11	12·20 G.
31	12·30	12	12·10
June 10	12·20	Aug. 11	12·00
30	12·4 ±	14	12·15
July 1	12·40	15	12·25

Nova Aquilæ (1918).

Observed on 22 nights, the magnitude being estimated on 21 of these. The observations indicate that the Nova has sunk to practically the same level of brightness as that from which it rose in 1918 June. Photographs taken previously were found to indicate an irregular variability over a range of a whole magnitude. Whether similar photographs taken at the present time would indicate such considerable changes I am unable to say; but certainly the visual range of variability, supposing the latter to be real, is much smaller, and can scarcely exceed a quarter of a magnitude. When examined with the 28-inch telescope on 1926 July 31, the image of the Nova was found to be perfectly stellar. No nebulosity was seen surrounding or attached to it, and the focus appeared to be the same as for other stars.

Date.	Mag.	Date.	Mag.
1926 May 3	10·6	1926 July 13	10·8
15	10·7	20	10·6
31	10·6	29	10·7
June 7	10·5	30	10·7
10	10·6	Aug. 4	10·8
29	10·65	11	10·7
30	10·55	21	10·7
July 1	10·6	26	10·7
7	10·55	28	10·8
9	10·65	Sept. 10	10·7
12	10·7		

Nova Aquilæ (1925).

Observed on 31 nights. This Nova is remarkable for the length of time during which it remained at or near its maximum, recalling in this respect the behaviour of Nova Pictoris. During the last three months of 1925 it had remained within $0^{m}.2$ of $8^{m}.8$, which was about its maximum brightness, and had only faded half a magnitude by the following March (see *M.N.*, **86**, 370). By 1926 May 15 it had again

risen to 8·8; but after this a steady fall set in, and the Nova lost one and a half magnitudes during June, July, and August. Seen with the 28-inch telescope on 1926 July 31, the image of the Nova appeared to be sensibly stellar in character; but it was closely surrounded by a nebulous glow, 3″ or 4″ in diameter, which I believe was objective. The focus seemed to be about 3 mm. longer than for other stars in the field.

Date.	Mag.	Date.	Mag.
1926 May 15	8·8	1926 July 15	10·0
31	9·2	20	10·1
June 5	9·0	29	10·2
7	9·0	30	10·4
10	9·2	Aug. 4	10·2
18	9·3	11	10·4
24	9·4	14	10·4
28	9·45	15	10·5
29	9·55	21	10·4
30	9·55	24	10·4
July 1	9·6	26	10·4
7	9·7	28	10·4
9	9·9	Sept. 4	10·4
12	9·9	10	10·5
13	9·9	Nov. 7	10·4

Nova Cygni (1920).

Observed on 11 nights. The fading of this object (which rose from below the 15th magnitude) is still continuing very slowly, at the rate of about $0^{m}.5$ per year. The fall seems to be smooth, and there is no evidence of any marked fluctuations. The 28-inch telescope shows no nebulosity round the Nova, and the image of the latter is perfectly stellar in appearance.

Date.	Mag.	Date.	Mag.
1926 Apr. 15	13·67	1926 May 31	13·77
16	13·67	June 13	13·77 G.
17	13·77	27	13·87 G.
May 3	13·72	July 9	13·85
15	13·82	Aug. 11	13·80
16	13·77 G.		

Nova Cygni (1876).

Observed on 15 nights. This is another of the Novæ that are certainly variable in light. It is, however, a faint object, and no aperture less than 12 inches can deal adequately with it. A range of

at least $0^{m}.7$ is indicated in the observations of 1926, made with the 28-inch telescope. This instrument shows no nebulosity round or near the Nova, and the image of the latter is stellar in character.

Date.	Mag.	Date.	Mag.
1926 May 15	14·71±	1926 July 9	14·91
June 13	15·00 G.	11	15·05 G.
21	14·33 G.	21	15·00 G.
26	14·57 G.	31	14·86 G.
27	14·71 G.	Aug. 1	14·96 G.
July 1	14·91	4	14·76 G.
3	15·0±G.	14	14·81
8	14·96 G.		

Nova Lacertæ (1910).

Observed on 4 nights. This Nova is steady in its light, or very nearly so. I think, however, that real changes, to the extent of about $0^{m}.2$, do occasionally occur. The average magnitude for many years past has been 14·1. On 1926 July 11 the Nova appeared, in the 28-inch telescope, to be involved in a faint nebulous glow, 3″ or 4″ in diameter. I am not entirely confident of the objectivity of this glow, but rather think that it represents real nebulosity. On this same night I had repeated suspicions of a companion of about the 16th magnitude in p.a. 100° ± ; distance 3″ ±. The image of the Nova was stellar.

Date.	Mag.	Date.	Mag.
1926 May 15	14·1	1926 July 1	14·1
June 10	14·2	11	14·3 G.

Observations of Novæ, 1926–1927. By B. M. Peek.

Photometric observations of four Novæ have been continued, and the resulting estimates of magnitude are tabulated below.

Nova Persei (1901). *Observed on 93 Nights.*

J.D. [2424]	Mag.	J.D. [2424]	Mag.	J.D. [2424]	Mag.
718·56	13·5 ±	749·46	13·42±	767·44	13·08
729·56	13·67	751·55	13·03	769·44	13·13
736·52	13·67	757·44	13·40	770·39	13·11
737·60	13·70	758·42	13·55	771·43	13·38
742·48	13·60	759·41	13·64	772·42	13·39
743·39	13·62	763·53	13·40	773·34	13·35
746·47	13·33	766·42	13·08	776·42	13·35

Nova Persei (1901). *Observed on 93 Nights*—continued

J.D. [2424]	Mag.	J.D. [2424]	Mag.	J.D. [1924]	Mag.
777·38	13·21	825·37	13·80	911·36	13·35 *
778·48	13·06	828·37	13·77	912·26	13·55
781·44	12·84	830·30	13·67	912·39	13·28 *
783·60	13·08	832·54	13·37	914·38	13·23 *
784·41	13·30	834·28	13·72	916·42	13·28 *
785·43	13·40	841·23	13·79	921·32	13·33± *
786·34	13·57	845·52	13·62	934·30	13·24 *
787·41	13·52	853·33	13·80	939·38	13·44 *
788·41	13·26	858·25	13·62	943·32	13·30 *
789·42	13·60	870·23	13·70	946·32	13·35 *
790·41	13·77	874·25	13·72	947·30	13·23 *
791·40	13·57	877·32	13·77	948·46	13·28± *
792·41	13·57	879·46	13·47	955·31	13·45± *
794·51	13·72	881·51	13·23	956·32	13·28 *
795·52	13·77			958·32	13·13 *
796·30	13·37	(1927)		960·32	13·23 *
797·31	13·55	882·30	13·77	963·40	13·18
798·38	13·60	882·47	13·47 *	966·42	13·31
799·36	13·77	884·41	13·52 *	967·45	13·30
806·41	13·61	885·50	13·54 *	968·32	13·28
807·37	13·64	[1924]		972·41	13·40
810·48	13·65±	886·27	13·71	974·36	13·38
814·32	13·57±	897·30	13·72	[2425]	
815·28	13·37	907·36	13·37 *	001·38	<13·18
821·37	13·77	908·35	13·25 *	002·40	13·52
822·41	13·77	910·25	13·60		
824·25	13·67	910·32	13·35 *		

Nearly all the observations were made with the 12-inch reflector. As it is now mounted, the eyepiece when near the zenith is much higher above the ground than it was at Boscombe, and when a high object is just past culmination the field assumes an unfamiliar orientation. From comparison of my own observations among themselves and with those of Dr. W. H. Steavenson, it seems fairly certain that in the case of the Nova Persei field the magnitudes of the Nova estimated when fairly high on the west of the meridian are too bright by about 0·25 mag. In the above table those observations to which this correction would be applicable are marked with an asterisk. Toward the close of the series the Nova was low enough for the eyes to assume their normal orientation.

Nova Ophiuchi (1848). *Observed on 35 Nights.*

J.D. [2424]	Mag.	J.D. [2424]	Mag.	J.D. [2424]	Mag.
653	12·65	713	12·25	769	12·45
667	12·35	714	12·35±	771	12·5
675	12·45	724	<12·3	772	12·45
677	12·45±	727	12·45±	776	12·45
678	12·3	729	12·2	777	12·4±
694	12·35±	736	12·25	784	12·35
697	12·15	737	12·2	786	12·35
706	12·05	742	12·25	795	12·05±
708	12·15	743	12·25	796	12·1
709	12·1	757	12·0	798	12·15
710	12·2±	758	12·3±	799	12·1
711	12·2	763	12·1		

Nova Cygni (1876). *Observed on 30 Nights.*

J.D. [2424]	Mag.	J.D. [2424]	Mag.	J.D. [2424]	Mag.
667	15·0±	769	14·96	824	14·86
706	14·9±	771	14·96	853	14·71
709	<14·5	772	14·91	858	14·76±
710	<14·71	786	15·06	874	14·66
711	<14·8	790	14·96	877	14·66
729	14·85±	792	14·96		
742	14·86	796	14·91	(1927)	
757	14·96	798	14·91	886	14·61±
758	15·0±	815	14·84	910	14·81
759	14·91	821	14·84	912	14·86
763	<14·71	822	14·81		

Nova Aquilæ (1925).

Observations of this star were continued, but as yet no authoritative magnitudes for the comparison stars are available.

For convenience of reference the following equivalents are given :—

Civil Date.	Julian Date.	Civil Date.	Julian Date.
1926 May 1	2424 637	1926 Dec. 1	2424 851
June 1	668	1927 Jan. 1	882
July 1	698	Feb. 1	913
Aug. 1	729	Mar. 1	941
Sept. 1	760	Apr. 1	972
Oct. 1	790	May 1	2425 002
Nov. 1	821		

Disturbed Periods in the Variation of R Aquarii. By P. M. Ryves.

1. R. Aquarii (2338*15*, Ch. 8512), a variable of the "long-period" class, was discovered by Harding in 1811, and is one of seven stars of this type which have been under observation for more than a century.

The magnitude at maximum is generally between $6\frac{1}{2}$ and $7\frac{1}{2}$, and since at minimum it seldom falls below $10\frac{1}{2}$, the light-changes can be followed with an instrument of quite moderate power. The position of the variable, however, south of both ecliptic and equator, is not favourable for observers situated in North temperate latitudes. In South Europe there is a period of about a hundred days each year during which observations cannot be made, entailing an inconvenient annual gap in the light-curve which an observer in the Southern Hemisphere might partly fill in.

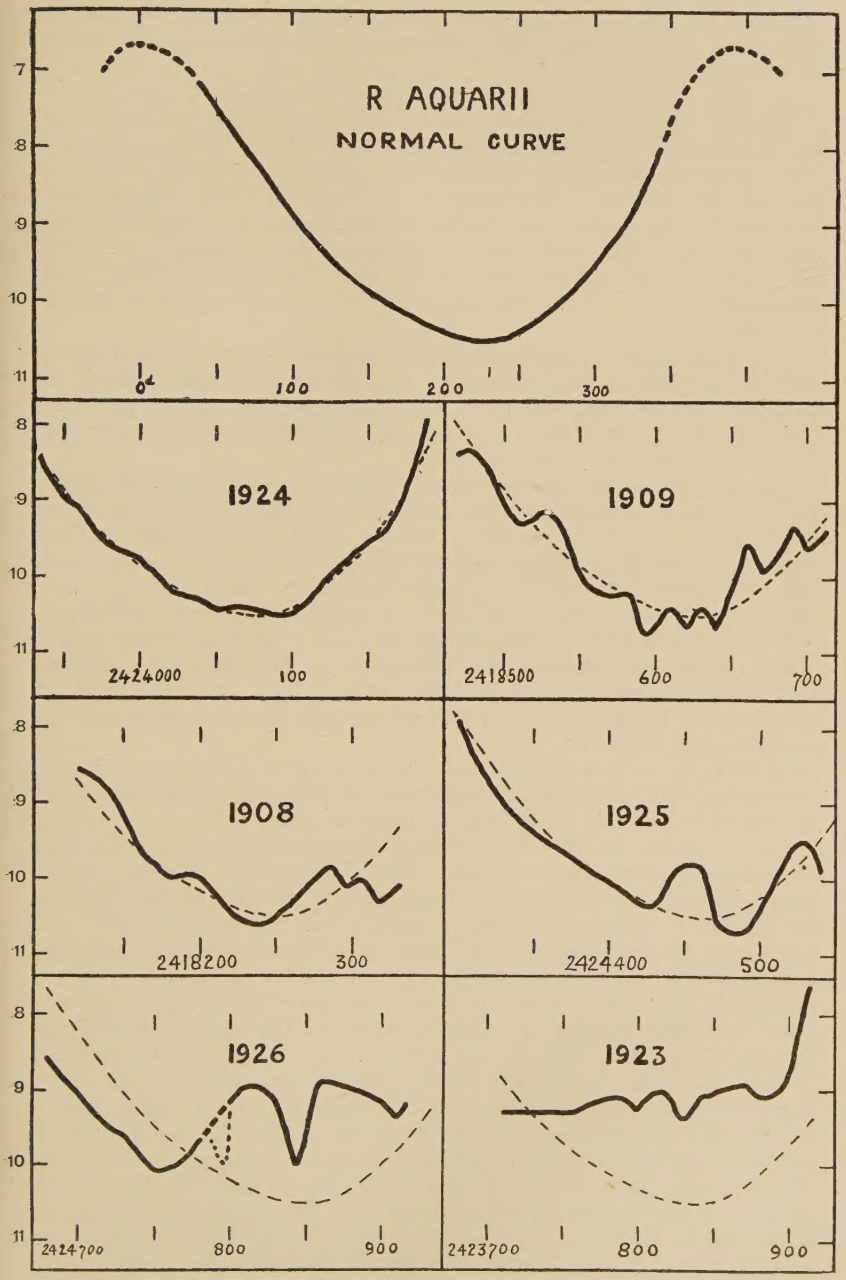
2. R. Aquarii has been observed by the writer since 1906, with a break of several years near the middle of the period dividing the observations into two groups. The estimates, with few exceptions, have been made by means of refractors of 3-inch and 5-inch aperture. The former instrument was used exclusively up to 1911, after which the 5-inch was frequently employed, and on a few occasions a 2-inch aperture.

The observations reveal irregularities in the variation of R Aquarii which are exceptionally large and of an unusual character for a variable of this type, sometimes amounting to an actual reversal in the direction of light-change. They appear to be specially prominent during the fainter part of the cycle, generally the most regular in long-period variable stars.

3. A period of 389 days suits the observations under discussion better than any other. This is nearly two days longer than that found by Chandler, who in *Third Catalogue* gives the elements: Max. = J.D. $2382847{\cdot}6 + 387^{d}{\cdot}16E + 35 \sin(10^{\circ}E + 235^{\circ})$. About the year 1911 the observations show that the maxima were taking place some 60 days earlier than the times indicated by the above formula, the sine term aggravating the discrepancy. If the longer period indicated by the observations be considered as the result of a temporary lengthening or a fluctuation around a mean value, a mean period of $386^{d}{\cdot}7$ would bring the observed maxima into fair agreement with Chandler's epoch. Recent maxima may be represented by the formula:

$$\text{Max.} = \text{J.D. } 2423451 + 386{\cdot}7E + 15,$$

the last term being an empirical factor introduced in order to bring the formula into agreement with recent observations without abandoning the mean period, which, in the long run, may be a significant value. The factor, altered from time to time in accordance with experience, may be used until a systematic term be discovered or the mean period overthrown. The same formula with *both* signs



R Aquarii.—Disturbed periods compared with "normal" curve.

(Scale: 1 cm. = 1 mag. = 50 days.)

reversed has been used in connection with the observations of the first group (1906–1913). This method has been adopted as a matter of convenience.

4. The upper figure in the accompanying diagram represents a " normal " curve of R Aquarii, not a mean curve in the ordinary sense of the phrase (a curve based on all the observations), but one obtained after excluding those observations made when the star was behaving in a peculiar manner, *i.e.* during what may be called " *disturbed periods.*" The curve refers to the years during which the star was under observation here, and is intended only as a standard for comparing the quiet or " normal " behaviour with that of the " disturbed periods," and principally during the fainter stages with which the present note is concerned.

The next figure (1924) shows the star behaving in a regular manner, the curve being almost coincident with the normal curve, which is shown by the broken line. In 1910, 1911, and 1912 the variation was of a very similar character as far as it could be observed (descending branch of curve and minimum phase).

In the 1909 curve the general tendency of the light-change is still fairly close to the normal, but there are secondary fluctuations on either side. In 1908 we have, besides slight irregularities in the descent, a definite reversal of direction in the rise and subsequent fall just after the calculated minimum phase. In 1925 the " disturbance " is still more remarkable, there being a double oscillation with an amplitude of a whole magnitude. In 1926 there was a somewhat similar disturbance, the minimum of the secondary oscillation being narrower in this case, and it may have been preceded by another, but as this latter depends on a single observation it may be considered as doubtful.

Finally, in 1923 R Aquarii failed to go down to a proper minimum, remaining, with only slight fluctuation, just below the ninth magnitude for nearly 200 days! After this the rise was rapid.

5. A typical variable star of long period may show considerable irregularities in its light-changes. The brightness at a given phase may be greater or less than the mean value ; the date of the phase as determined by observation may be early or late on the time-table, and the shape of the light-curve different at different cycles ; but well-authenticated cases of an actual reversal in the direction of light-change are very rare ; moreover, the irregularities mentioned are always more noticeable during the brighter stages of the cycle, especially in the case of a star exhibiting the general characteristics of R Aquarii : broad minimum, considerable range, long period, etc. As the light fades there ensues a period of inactivity during which the light-change is both slow and regular. The star appears to subside into a state of repose from which it awakens, gradually at first, then more rapidly, after the minimum phase is passed. In R Aquarii we find this rest-period frequently interrupted by disturbances ; or the usual decline to minimum may be arrested. It would seem that the equilibrium of the star is very unstable even during this period and may be easily upset, recrudescences of activity taking place.

These phenomena, observed photometrically, receive confirmation from the spectroscopic evidence of bright lines about the time of minimum recently obtained in America. It would be interesting to know whether there is a clear correlation between the individual secondary fluctuations and the appearance of the bright lines or other changes in the spectrum.

Zaragoza:
1927 *May.*

Errata.

Mr. Perrine's paper on Distances of the Galactic Cepheids. P. 427, in Table I. the word "Brightest" does not apply to Group F6–Go, but only to the two following groups. P. 429, *for* "These distances do not all agree," *read* "These distances do not at all agree."

Dr. Comrie's paper on Eclipses. P. 484, the formula for the maximum angular breadth of the illuminated portion should be—

$$\frac{2(L + m - 0{\cdot}5459)}{2L - 0{\cdot}5459} \times \text{Sun's semi-diameter.}$$